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I. AN EXAMINATION OF THE THEORIES THAT HAVE BEEN PROPOSED TO ACCOUNT FOR THE CLIMATE OF THE GLACIAL PERIOD.

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IN the speculations I ventured to make in a recent work* on some of the phenomena of the Glacial period, I purposely avoided entering on the question of the cause of the great accretion of ice, believing that the time was not ripe for its discussion, and hoping that it might be taken up by some astronomer, as it is to Astronomy rather than Geology that we must look for a solution of the problem. I find, however, that my explanations of the facts of the "great ice age" are constantly met by objections founded on the theories of the cause of that event; and I propose in the present paper to discuss the principal hypotheses that have been advanced to account for the origin of the Glacial period, and to endeavour to show that my speculations on the extent and effects of the ice are in accordance with, and a necessary consequence of, the theory that is most in harmony with the facts with which we have to deal.

1. *Theory of a Change in the Relative Position of the Continents and the Ocean.*—In that great work the "Principles of Geology," in which the foundations of the modern science were laid in 1830 by Lyell, and in successive editions in which the veteran philosopher has ever kept abreast of advancing knowledge, he has brought forward and supported the theory that great oscillations of temperature have been produced by changes in the relative positions of land and water. This theory he has enforced with a wealth of illustration derived from his vast acquaintance with geological and geographical facts, and by the masterly arguments of a clear, comprehensive, and judicial mind. Chiefly through

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* The Naturalist in Nicaragua, p. 262.

his powerful advocacy, after nearly half a century has elapsed, it still holds its ground amongst the rival views that have been advanced, and deserves our first consideration.

Lyell takes for his starting-point the undoubted fact that the sea and the land are now in some parts changing places. Along some coast lines the land is either slowly sinking or has sunk in post-glacial times, whilst in others the continents have been raised above their former level. He proceeds to show that the climate of a place is greatly dependent upon its position with respect to great masses of land or water; that an insular climate is less extreme than that of the interior of a great continent; and that currents of water from the tropics, or from the arctic regions, are very effectual in raising or lowering the mean temperature above or below what is due to distance from the equator alone. He then considers what change in the relative position of land and water would produce the warmest and what the coldest climate, and comes to the conclusion that if all the land was distributed around the equator we should have the warmest climate possible due to geographical conditions, and that if all the land was situated at and around the poles we should have the extreme of cold.

There can be little doubt that if the second set of conditions prevailed, or even some approach to them, they would be effectual in rendering more severe the climate of polar regions, and in causing a greater accretion of ice than now prevails. A rise of polar and a submergence of tropical lands would certainly lower the temperature of the arctic regions. A mere rise of land, sufficient to close Behring's Straits and to connect America through Newfoundland with Europe, would, by shutting off all warm currents from the polar seas, tend to a greater accumulation of ice, as the heat of the Gulf Stream and other warm currents—that is now expended in tempering arctic seasons and melting polar ice—would then cause greater evaporation, and consequently greater precipitation, on the frozen lands of the north. But it must not be forgotten that the warm currents flowing northwards are counterbalanced by cold ones flowing southwards; and if, on the one hand, regions enjoying a warmer climate through the influence of the Gulf Stream would then be shut off from its influence and subjected to greater cold, so, on the other, coasts—such as that of eastern North America—now cooled by polar currents, would be laved by warmer waters. Yet it is on the eastern side of North

America that the ice extended farthest towards the equator in the Glacial period.

When Lyell first propounded his theory geologists were very imperfectly acquainted with the facts that were to be explained, and it was thought that if it could be shown that by an alteration in the configuration and distribution of land, and a change in the direction of the currents of the ocean, icebergs might be floated down to the latitude of London, lowering the temperature as they do now in South Georgia in lat. 54° S., so as to allow of a perpetual covering of snow and the existence of glaciers on the higher grounds, a satisfactory solution of the problem would be arrived at. But in the half century that has nearly passed since then our conceptions of the extent of the ice of the Glacial period have slowly but greatly expanded, and we know now—although many English geologists still close their eyes to the evidence—that the problem to be solved is not one of icebergs floating over submerged lands, but a vast piling up of ice and snow around the poles, that accumulated until it flowed outwards over the existing continents. Let us trace this great ice-sheet round the northern hemisphere, as we are now nearly enabled to do by the latest observations on its extent in northern Asia.

Commencing in North America, we learn from Dana and other eminent American geologists that to the north of the St. Lawrence the ice was at least 12,000 feet, or 2½ miles, in thickness; in the northern parts of New England was over 6000 feet in thickness, and, gradually thinning southwards, reached in the lower grounds the parallel of 39° N. in the southern parts of Pennsylvania, Ohio, Indiana, Illinois, and Iowa, whilst along the mountain ranges local glacier systems reached in the tropics at least as far as Nicaragua, where within 13 degrees from the equator I found undoubted traces of glacier action reaching to 2000 feet above the sea-level, where snow now never falls.

Coming eastward we find, in Nova Scotia and Newfoundland, everywhere evidence that they were completely overwhelmed with ice. Iceland, according to Robert Chambers, is scored across from one side to the other, and was buried in ice that may have reached the British Isles, for the Hebrides and the north-eastern extremity of Scotland were overflowed by ice that came from that direction. This ice, overflowing Caithness, joined by great streams from Scandinavia, and further reinforced by glaciers from the mountains of Scotland and the north of England, pushed down the bed of the German Ocean, reached as far as the coasts of

Norfolk, and thrust up great masses of chalk and other angular rocks upon the land. We have a measure of its thickness in Southern Yorkshire, and learn that it was not so deep on the eastern as it was on the western side of England, for the drift does not reach higher than 600 feet above the sea, excepting where the Wye, the Calder, and the Aire cut through the Pennine Chain, and form passes through which the ice streamed from the west, where it was much higher. The Irish Sea was filled with it, flowing southwards, at least 2000 feet thick. It butted against the Welsh mountains, and, dividing, one part pushed up the valleys of the Mersey and the Dee, and through what has been called the Straits of Malvern, certainly as far as the water-shed of the Severn, probably as far as the Bristol Channel; the other and larger stream, shouldering the western slopes of the mountains of Cardiganshire, flowed across Anglesea to the Atlantic. In Ireland the ice was still thicker, and Mr. Campbell considers that in the extreme south of that island he has obtained proofs of it having been at least 2000 feet thick. This thickening of the ice westward proves that the British Isles were not glaciated from Scandinavia.

Passing across to the continent we find Scandinavia hugely glaciated, and that the ice-sheet that flowed from it filled the Gulf of Bothnia and the Baltic. Denmark was assailed by the advancing ice, and everywhere traces are left of its vast extent and force. In the island of Möen the chalk strata are dislocated and folded together, inclosing in their folds patches and seams of boulder clay. The Danish geologists have ascribed these to a faulting and bending of the strata since the Glacial period; but both in Nova Scotia and at Abergairn, in Aberdeenshire, I have seen great masses of strata that have been pushed along horizontally over others by the great force of the advancing ice, and think that the post-tertiary contortions of the strata in Möen must be due to the same agency. After crossing the Baltic the ice crept southwards, and all over northern Germany and Holland blocks of stone strew the surface that have been brought by it from the mountains of Norway and Sweden. It reached its southern limit somewhere about Antwerp, and eastward the range of the northern drift has been traced to an irregular line across the Continent. In European Russia the ice reached to Nijni Novgorod, in lat. 52°; to which parallel I have also traced it in north-western Asia, near Pavlodar, in Siberia, and in a paper read before the Geological Society of London have described the facts that have led me to the conclusion that the ice from the north blocked up the whole water-shed of Siberia as far as the borders of Kamtchatka.

We thus find everywhere in the northern hemisphere that the ice thickened northwards, that it radiated from the pole; and that its margin nearly girdled the world, and probably would be found to have done so completely if there were land to preserve its traces.

There are many geologists who believe that these northern lands were not all glaciated at the same time,—that, for instance, the Glacial period of North America was not contemporaneous with that of Europe. Those who thus argue adopt, in some form or other, Lyell's theory that the cold of the Glacial period was produced by a change in the distribution of land and water. Thus Mr. Hopkins, in 1852, calculated that if—by some change in the relative position of sea and land—the Gulf Stream could be diverted from its present northerly course, whilst northern and western Europe were submerged to the extent of 500 feet, and subjected to the influence of a cold current passing over the depressed area, the snow-line would descend to 1000 feet above the sea-level in Wales and the west of Ireland, and glaciers would reach the sea. Although this amount of change would be totally insufficient to account for the facts of the Glacial period, it may still be useful to point out that not a single scrap of evidence has been adduced to show that the Gulf Stream ever passed over any portion of Europe or America that is now dry land.

Throughout the whole of the Tertiary period the continents appear to have had much the same area and figure as they at present possess. Dana has also pointed out that, even so far back as the Jurassic period, the Gulf Stream exerted the same kind of influence upon the temperature of the North Atlantic as it does now. He considers that the existence of corals in the English oolites proves that the coral reef boundary extended 22 degrees of latitude beyond its present farthest northern point, and believes that the Gulf Stream must have aided in this result. Other facts indicate its existence and influence in cretaceous and tertiary times,—as, for instance, the representatives of the French Faluns on James's River, in North America, denote a cooler climate in lat 37° N. than prevailed at the same time in lat. 47° N. in Western Europe,—whilst in the glacial epoch the extent of the ice in Western Europe and Eastern North America curiously and suggestively conforms with the curve of the present isothermal lines due to its action. Just as now, the isotherm of 50° F., passing across the south of England near the latitude of London and the Bristol Channel, sweeps south-westwardly across the Atlantic, and reaches to about Baltimore, in North America, so in the Glacial period the margin of the ice, flowing southwards, attained nearly the same limits; indicating that the warm waters from the tropics then, as now, were deflected against the western coasts of Europe by the rotation of the earth, and gave them a higher temperature than the same latitudes on the eastern coasts of America. The sea teems with life, and it is not possible that this current could have flowed over any part of Europe without leaving many memorials of its course behind it. But even if it had been diverted, and a cold current brought icebergs from the Arctic regions past the British Isles, how could that, or any modification of such a theory, cause continental ice to reach the sea-level in lat. 39° in North America? I cannot imagine any alteration of the present coast-lines that could cause a greater curve in the isothermal lines than at present exists in the North Atlantic; and to assume that during the Glacial period the warm and cold currents shifted their position all round the hemisphere, so as to bring every part, at one time or other, within a greater extreme of cold than now anywhere prevails, is to call for an amount of movement in the earth's crust that no evidence warrants nor analogy suggests.

Whilst Lyell, in his latest works,* adheres to his opinion that former changes of climate have been chiefly governed by geographical conditions, he candidly admits that since he first attempted to solve the problem, our knowledge of the subject has vastly increased, and that it has assumed a somewhat new aspect, so that he now considers it probable that astronomical causes may have combined with geographical changes to produce an exaggeration of cold in both hemispheres. The principal of these astronomical theories I shall now take into consideration, but I shall have in the sequel—when I come to show what bearing the facts of the Early Tertiary period have on the discussion—to make some further remarks upon the insufficiency of geographical changes to account for the great oscillations of temperature of which we have geological proofs.

2. *Theory of an Increase of the Ellipticity of the Earth's Orbit.*—Mr. Croll, in a series of papers published in the "Philosophical Magazine," has advocated, with great ability

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* Principles of Geology, 1872, pp. 173 and 284.

and learning, and strengthened by laborious calculations, the theory that the cold of the Glacial period and the warmth of other geological epochs were due to great changes in the ellipticity of the earth's orbit. As has long been known, the earth, in its annual course around the sun, does not describe a circle, but an ellipse, and is much nearer to the great luminary in some parts of its course than in others. Astronomers have also proved that the eccentricity of the orbit varies during vast periods of time, and that at its greatest eccentricity—one of which periods happened about 200,000 years ago—the earth in aphelion was nearly 98,500,000 miles distant, whilst now when in aphelion it is about 90,000,000 miles from the sun.

One result of the eccentricity of the orbit, combined with the obliquity of the ecliptic, or the angle that the axis of the earth makes with the plane of its orbit, is, that at present the sun is north of the equator about 7½ days longer than it is south of it. But as at the time the sun is south of the equator the earth is nearest the source of heat, the southern hemisphere receives just as much heat in its shorter summer solstice as the northern hemisphere does in its longer one. Astronomers have calculated the effect of a much greater eccentricity of the orbit, and have unanimously come to the conclusion that the absolute amount of heat received by the two hemispheres would be the same, however great that eccentricity might be. But as the total amount of heat received from the sun is inversely proportional to the shortest diameter of its orbit, it follows that during the periods of greatest eccentricity the absolute amount of heat received by the earth, and distributed equally to the two hemispheres, would be slightly in excess of that received when the eccentricity was much less.

The general conclusion arrived at by astronomers before Mr. Croll examined the problem—including the eminent names of Humboldt, Arago, and Poisson—was that the climate of our globe could not be affected by any possible change in the ellipticity of its orbit. In this opinion Herschel—who at one time thought that great changes of climate might be so produced—appears afterwards to have coincided. Mr. Croll, however, states that in arriving at this conclusion a most important element of the enquiry had been omitted. Fully admitting that the absolute amount of heat received in the two hemispheres would be the same, however great the ellipticity might be, he yet urges that in that hemisphere in which the nights were longest there would be most heat lost by radiation, and in that way the mean temperature would be greatly lowered.

Mr. Croll puts his theory briefly in these words:—"The southern hemisphere is further from the sun during its winter than the northern, and therefore cools more rapidly. It is, however, nearer to the sun during its summer than the northern, and on this account cools more slowly. The heat thus saved during summer would exactly compensate for that lost during winter were the two periods of equal length; but as the southern winter is longer than the southern summer by more than 7½ days, there is on the whole a greater amount of heat lost during winter than is saved during summer." "The greater length of the winter half year over the summer half, when the eccentricity is near its maximum, would affect the climate in two different ways:—(1), by allowing the ground to cool by radiation to a greater extent than it would otherwise do were the (summer) seasons of greater length; and (2), by lengthening the ice-accumulating period and shortening the ice-melting period. The influence of the first cause upon the glaciation of the country would probably be felt to a considerable extent; but it is to the second that we must attribute the principal effect."* The above was written in 1865, but I cannot find that Mr. Croll has modified his theory in any later writings; and Mr. James Geikie, his colleague on the Geological Survey of Scotland, has, during the present year, in his work "The Great Ice Age," adopted it, and in discussing it has described it substantially as above. Now if it be true that the hemisphere, that has its winter when the earth is farthest from the sun, will have its mean temperature reduced by an excess of radiation; whilst that of the opposite hemisphere will be correspondingly increased, we have certainly a true cause of former great oscillations of climate. Before, therefore, entering on the consideration of some other causes that would, according to Mr. Croll, be brought into action and intensify the effects, it will be well to examine the fundamental bases of the theory.

1st. Would there be more radiation of heat into space, and consequently increased cold, at times of the greatest ellipticity of the orbit in that hemisphere whose winter happened when the earth was furthest from the sun? Mr. Croll, as we have seen, answers in the affirmative, and in the shape in which he puts it it appears as if it would be so. As at that time the number of hours of night in each year were much more in one hemisphere than the other, it is quite certain that more heat would be radiated during the nights that were longest. But, and this is the fallacy on which it seems to me Mr. Croll's argument rests, the earth radiates heat in the day time as well as at night, and this has not been taken into consideration. The warmth of the day depends on the excess of heat received over what is radiated, not that there is no radiation at that time; and if we take into account the heat radiated during the day we shall find that no more is lost in one hemisphere than the other from that cause. And if the absolute amount of heat received from the sun be equal whatever the amount of ellipticity, and the absolute amount of loss by radiation also equal when we calculate that radiated during the day as well as that during the night, it is evident that the absolute difference between the heat received and the heat lost, or the mean temperature of the two hemispheres due to these causes alone, must be the same whatever the amount of ellipticity of the orbit may be.

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* Reader, 1865, p. 631.

2nd. Would the lengthening of the ice-accumulating period and the shortening of the ice-melting period cause a greater accretion of ice? Here again Mr. Croll and his followers answer unhesitatingly in the affirmative, and they put it in this way:—"At the time of greatest eccentricity during the long winter of aphelion, longer by thirty-six days than the summer of perihelion, such an accumulation of snow and ice would have taken place that even the diminished distance between the earth and the sun in summer time would be powerless to effect its removal."* Here, again, I think the argument is based on a misconception. It is not a fact that our winter begins as soon as the sun has passed the autumnal equinox, though what is called the winter solstice does. The nights are longer than the days, but snow does not immediately begin to fall nor water to freeze, and our winter does not commence on the 22nd of September, but several weeks later. In the shorter but hotter summer of perihelion some excess of heat must be stored up in the earth, the sea, and the atmosphere, not to be entirely given up until long after the winter solstice has been entered on. The advocates of this theory affirm that the mean temperature would be lowered because the heat of the short summer would be taken up in melting the ice that had accumulated in winter, but a pound of water in passing from a liquid to a solid state evolves just as much heat as is required to melt it again, and the heat given off in winter by the freezing water is equal to that absorbed when it melts again, so that the mean temperature is not affected.

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* The Great Ice Age. By James Geikie. 1874. P. 139.

Again, it is said that clouds would accumulate around the pole with its winter in aphelion. Why they should do so does not appear very clearly, but clouds would receive the rays of the sun on their upper surface, and in some way or other the heat would be utilised in ameliorating the climate; nor should it be forgotten that clouds prevent radiation during the night as well as intercept the sun's rays during the day.

There is, however, a cause not touched upon by Mr. Croll that does act in preventing the excess of heat of summer counterbalancing its diminution in winter where snow covers the ground. It is not because the heat is used up in melting the snow, but because much of it is not so used up but is reflected back into space from the white surface. If it were not for this, snow would nowhere be perennial, but everywhere the heat of summer would dissolve the snows of winter; and if, without taking into account any lengthening of the winter by reason of the ellipticity of the orbit, the whole of the winter solstice were an ice-accumulating period it would now gather year by year until it overwhelmed the temperate zones, because the six months' snow would reflect so much of the other six months' heat that it would not be melted but would gradually accumulate. It does not do so, because only at the very poles are there six months winter and six months summer, and the ice-accumulating period gradually decreases when we leave the poles, and reaches zero long before we arrive at the tropics. These conditions were probably the same at the time of greatest ellipticity, and at the most only a very small amount of heat could be lost by reflection from snow-covered lands more than now: and as at that time—according to the law that the amount of heat received is in inverse proportion to the length of the shortest diameter of the orbit—there would be a slight increase in the absolute amount of heat received from the sun, it is probable that one would counterbalance the other; and I cannot but come to the conclusion that Arago was right when he affirmed that even if the ellipticity of the orbit was much greater than astronomers have shown to be possible, "still this would not alter in any appreciable manner the mean thermometrical state of the globe."

Mr. Croll has sought to strengthen his theory by endeavouring to show that other physical causes would be brought into operation during a great ellipticity of the orbit which would tend to decrease the temperature of the hemisphere that had its winter in aphelion, and to increase that of the other. The most powerful of these he considers would be a change in the great currents of the ocean by which at present a large amount of heat is conveyed from the tropics to the poles. He maintains that these currents are produced by the trade-winds, and that when the temperature of one hemisphere was reduced and the other increased in the manner and by the causes already discussed, the trade-winds on one side of the equator would be weakened and on the other strengthened, and in consequence the warm currents flowing towards the poles would in one hemisphere be augmented and in the other decreased, if not stopped altogether. For instance, he considers that the Gulf Stream is produced by the action of the trade-winds, and that in case of a great ellipticity of the orbit when the winter of the northern hemisphere happened in aphelion the air would be chilled, whilst that of the southern hemisphere would be warmed, and thus the aërial currents flowing from the poles towards the equator would be altered. Under these circumstances "the winds from the severe wintry north would sweep with much more vigour towards the equator than the opposite winds from the south pole. And hence Mr. Croll contends that with weaker winds blowing from the south the great antarctic drift-currents would be reduced in volume, while the subsidiary currents to which they give rise, namely, the broad equatorial and the Gulf Stream, would likewise lose in volume and force. And to such an extent would this be the case that, supposing the outline of the continents to remain unchanged, not only would the Brazilian branch of the equatorial current go on at the expense of the Gulf Stream, but the Gulf Stream he thinks would eventually be stopped, and the whole vast body of warm water that now flows north be entirely deflected into the southern ocean."* Well may Mr. Geikie say that the effect of the withdrawal from the north of all these great ocean rivers of heated water would be something enormous.

But is Mr. Croll's theory of the origin of the Gulf Stream correct? Is it possible to believe that the great body of water in the Atlantic Basin would be warmed at one end and cooled at the other without some system of circulation being set up? If currents in the air are caused by the unequal heating of different portions of it, why should not currents in the ocean be in like manner set in motion? Mr. Carpenter contends, and has illustrated by experiment, that they are; and if he be correct, instead of the Gulf Stream being lessened by the increase of ice in the north, it would be greatly augmented; and I have already shown that there is evidence of its existence and influence in the glacial period.

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* The Great Ice Age, p. 142.

Another cause that Mr. Croll thinks might be a means of increasing the vicissitudes of temperature produced by the eccentricity of the orbit, is a change in the obliquity of the ecliptic. Accepting the conclusions of some eminent astronomers that the obliquity of the ecliptic can only vary to a small extent, he yet considers that this small amount would cause a great change of temperature; that when the obliquity was at its maximum, or, according to Laplace, 24° 50′ 34″, there would be an increase of temperature at the poles equal to 14° or 15° if they were not covered with ice, but if they were, then the total quantity of ice melted at the poles would be one-eighteenth more than at the present.* On the contrary, when the obliquity was at its minimum, there would be a decrease of temperature at the poles and an increase of the ice covering them. This struck me when I first read it as a most extraordinary conclusion, and I considered it must have been the result of an inadvertence, as it appeared obvious that the effect would be just the reverse of that stated. But I find that Mr. Geikie, in his recent work, follows Mr. Croll in this as in other matters, and states that "if the obliquity of the ecliptic reached a minimum during our glacial epoch, as indeed it must have done more than once, the effect of the great eccentricity and diminished obliquity combined would be to intensify the glaciation of our hemisphere."†

As, in the former argument, I have had occasion to show that the radiation of heat by the earth during the day had been neglected, so in this calculation the all-important fact has been overlooked, that if the obliquity of the ecliptic be increased, the arctic circle will be enlarged and a greater area of the earth's surface brought within the influence of the long arctic night. A diminished obliquity, on the contrary, would lesson the difference in the temperate zones between the length of the night and day, and in so far moderate the extremes of cold and heat in winter and summer. The fallacy of the argument can, however, be best shown by considering what would be the effect of diminishing the obliquity to zero. When the direction of the axis of rotation of the earth became perpendicular to the plane of its orbit, the difference of the seasons of the year would disappear and perpetual spring would reign in the arctic regions. All over the globe there would be twelve hours night and twelve hours day, and no amount of ellipticity of the orbit could have any effect in lengthening the nights and days. Every step in the diminution of the obliquity of the ecliptic would be an approach towards this state of perpetual equinox, and tend more or less to equalise the seasons. The theory of Mr. Croll is based on an assumed exaggeration, by increased eccentricity of the orbit, of the effects of the present obliquity of the ecliptic, and it is startling to find it urged that a decrease in that obliquity would increase the results.

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* Philosophical Magazine, vol. xxxiii, p. 436.

† Great Ice Age, p. 147.

Having thus shown that the foundations of the theory present many points of weakness, I shall next take into consideration the question of how far it is in harmony with the geological facts sought to be explained by it. One of the points insisted upon by Mr. Croll, and which is stated to be in accordance with the facts known to geologists, is that during the greatest eccentricity of the orbit periods of glaciation would alternate with others of great warmth. Whilst one hemisphere was undergoing the extreme rigour of a glacial period, the other would rejoice in a "perpetual summer." And, owing to the precession of the equinoxes by which there is a complete revolution of the equinoctial point in 21,000 years, in half that time the hemisphere that had its winter in aphelion would slowly change until it had it in perihelion. The ice that had been heaped up at one pole would melt away and be piled up at the other. And as the last greatest period of elllipticity occupied, according to Mr. Croll's laborious calculations, about 160,000 years, there would during that time be several complete revolutions of the precession of the equinoxes, so that each hemisphere would have alternately several glacial periods and several warm periods.

To prevent misconception I shall give Mr. Croll's opinion on this question in his own words. He says:—"It is physically impossible that we can have a cold and arctic condition of climate on the one hemisphere, resulting from a great increase of eccentricity, without at the same time having a warm, equable, if not an almost tropical, condition of climate prevailing on the other hemisphere." "If the

Post Pliocene period afforded no geological evidence of a warmer condition of climate in Europe than now prevails, it would be so far a presumptive evidence against the assumption that the glacial epoch resulted from cosmical causes." "If it should actually turn out that there is no such thing as a warm and equable condition of climate somewhere about the time of an ice period, then the whole theory would have to be given up, because a warm period according to theory is just as necessary a result of an increase of eccentricity as a cold period."*

Now not only would the periods of great cold alternate in each hemisphere with periods of "perpetual summer," according to this theory, but as the ellipticity of the orbit approached its greatest eccentricity, warm or genial climates would alternate with colder ones, the extremes becoming more and more marked as the time of greatest eccentricity was neared. We ought therefore to find before the Glacial period evidence of great changes of climate, alternations of warm and cold periods, in the successive faunas, of which we have the records preserved in the Tertiary rocks. Instead of this, there are proofs of the gradual and continual decrease of temperature in Europe from the earliest Tertiary times. According to Lyell, "as we ascend in the series, the shells of the successive groups of strata—provincially called 'crag' in Norfolk and Suffolk—are seen to consist less and less of southern species, whilst the number of northern forms is always augmenting, until in the uppermost or newer groups, in which almost all the shells are of living species, the fauna is very arctic in character, and that even in the 52nd and 54th degrees of north latitude."† And if we go back to earlier Tertiary times than the Crag period, we find all the faunas—back to the very commencement of the Tertiary formations—evidencing warmer and warmer climatic conditions as we recede from the Glacial period. Nor is this evidence confined to the faunas; it is perhaps even better illustrated if we trace the successive floras from the Eocene upwards to the Glacial period. Commencing with the Lower Eocene we find in the London clay the fruits of numerous palms, belonging to genera only now found in the tropics, accompanied by the custard apple, gourds, and melons. These are followed in time by the Bournemouth beds, with subtropical Proteaceæ, numerous fig-trees, the cinnamon, and many other plants and trees, reminding the botanist of parts of

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* Philosophical Magazine, vol. xxxvi., page 380.

† Principles of Geology, p. 199.

India and Australia. In the Lower Miocene beds of Switzerland, the flora of which has been wonderfully preserved on the northern borders of the Lake of Geneva, there are still species of fig, cinnamon, palm-trees, and other subtropical vegetation, but with them appear species of poplar, hornbeam, oak, elm, and other trees now characteristic of temperate climes, which are absent from the European Eocene strata, and which indicate a less tropical climate. These beds are succeeded by the Upper Miocene strata of Œningen, still containing many exotic genera, but with a still larger proportion of species that betoken that the climate—though still more equable and warm than at present—was gradually becoming unsuitable for subtropical plants. Coming still higher in the Tertiary series, we find in the Lower Pliocene of Italy that most of the subtropical genera have disappeared, and when we reach the Newer Pliocene deposits the trees and shrubs are those now characteristic of European forests, and suggest that the climate was similar to that at present enjoyed in Europe. Then in England we find the Newer Pliocene beds, with their trees and plants of recent species, as in the Cromer Forest bed, followed by lignite beds at Bure and Westleton, containing *Salix polaris*, now only known within the arctic circle, and *Hynum turgescens*, an arctic moss. M. Nathorst, a Swedish geologist, who has studied these beds, considers that there is a gradual passage from the mild period of the forest bed, probably only a little colder than at present, to severe arctic conditions. These Bure and Westleton beds are succeeded by the till and boulder clay of the Glacial period.

If instead of the successive floras we follow the successive faunas, the land animals or the marine, we have a precisely similar succession of events, a gradual transition from the tropical forms of the Eocene and the subtropical ones of the Miocene through the more temperate species of the Pliocene, up to the arctic shells and mammals that usher in the Glacial epoch. The evidence is complete that points to the gradual cooling of the climate, and there is none whatever to show that there were any alternations of cold and warm periods. It is exactly the same kind of evidence as we should have if we travelled from the tropics along the coast of the continent of America northwards. The plants and land animals on the one hand, the inhabitants of the deep on the other, would gradually change their character; tropical forms would give place to subtropical, these to temperate, and finally, when the far north was reached, arctic species would predominate.

Mr. Croll has pointed out that though we have no evidence to support his theory in the successive faunas and floras of the Tertiary strata, yet in the Eocene beds of Switzerland and the Miocene of the north of Italy there are conglomerates containing large transported blocks of stone. Fully admitting that these were most likely transported by ice, I need scarcely remind geologists that no marine remains have been found with them, and that they were probably deposited in lakes, for although the Miocene boulder beds of Piedmont are more than 100 feet thick they contain no organic remains, and we know that this is a feature of modern glacial lakes. The beds rest also on Lower Miocene strata, mostly of fresh-water origin. To adduce such isolated facts as proofs of the existence of Glacial periods in Early Tertiary times is as logical as it would be to argue that there is now a Glacial period in the tropics because there are glaciers there. It is as if a traveller on the coast of western tropical America, coming in sight of one of the snow-capped summits of the Andes, should contend—although the sea and the land teemed with tropical forms of life—that he was in the arctic regions. Probably throughout geological history there never was a time when some mountain summits did not rise above the limits of perpetual snow, and we may expect to find in every geological formation some ice-borne boulders, without being forced to conclude that it required a Glacial period to transport them. The only safe guides to follow are the fauna and flora preserved in the strata, and even these fail us when we go far back in geological time, for we know not what to call tropical and what temperate forms; but so far as Tertiary rocks are concerned we may accept their evidence, and they prove that there were no oscillations between extreme heat and extreme cold, but a gradual and continuous decrease of temperature from the Eocene up to the Glacial period.

Coming to the Glacial period itself, what evidence have we of the intercalation of that time of "perpetual summer" that is, according to Mr. Croll, a necessary consequence of his theory? The fact most commonly appealed to, both on the Continent and in England, in support of this supposition, is the presence of seams of lignite in Switzerland—as at Dürnten, in the canton of Zurich—resting on a great thickness of boulder clay, and capped by beds of gravel with large erratic blocks. These seams of lignite generally vary from 2 to 5 feet in thickness, but in some parts swell out to as much as 12 feet. I admit that the evidence is conclusive that after the ice—during the great extension of the Swiss glaciers—had occupied the ground for a long period, it retreated, and peat mosses accumulated in low swampy spots; but I dispute that there is any evidence of a warm climate. Cones of the Scotch and spruce firs, and leaves of the oak, the ash, and the yew, have been found in these deposits, and, as these are all of existing species, Prof. Heer has inferred that the climate was similar to that now experienced in Switzerland. In reality it may have been colder, for all these trees range to more northern latitudes. The bones of the large Mammalia found in the same deposits tell us nothing of the climate, or, at the most, do not throw any further light on the question than is derived from a study of the vegetable remains. All that is proved is, that towards the latter part of the glacial period the ice retreated, and after a long interval advanced again, and covered some great mosses that had accumulated during its retreat. We have had a similar event, though on a smaller scale, in historical times. M. Venetz has pointed out that before the tenth century the Swiss glaciers extended further than they now do, that then for four centuries they gradually melted back, and then again began slowly to advance, and have been ever since gradually regaining their lost territory. If this be so, they must have passed over surfaces on which vegetation grew during their retreat, and if these surfaces were again uncovered we might find leaves of existing Swiss trees in deposits between two sets of Moraine gravels, one of an earlier and one of a later date than when the trees flourished.

Mr. Croll has himself advanced, as a crucial test of his theory, that as whilst one hemisphere was being glaciated the other was enjoying an almost tropical climate, and that as these conditions alternated several times during the period of greatest eccentricity, we ought to find proofs of the existence of these warm periods intercalated with those of greatest cold. And the evidence we require is, not that firs, oaks, and yews grew in Switzerland, as they do now, on moraines, during a temporary retreat of the ice, but of species that now live much further south, having then advanced far northwards. In fact, we want evidence, such as we have seen is so abundant in the Miocene strata, of a subtropical fauna and flora having flourished in Europe in interglacial times, and nothing less is satisfactory, according to this theory. The periods of greatest heat are as necessary a result of the theory as those of greatest cold, and they ought to occur alternately.

I fully believe that if any one takes the trouble to read this paper, in future years, they will think many of my arguments unnecessary and superfluous; but my contemporaries know what a large amount of acceptance this theory has met with amongst our leading scientific men, many of whom have adopted it as the true cause of the Glacial period. What is required, therefore, at the present time, is a thoroughly exhaustive examination of it, and to the best of my ability I shall make it. The most complete geological evidence is that of the marine shells. They have been more certainly and abundantly preserved than other organisms, and from the earliest Tertiary epoch up to the present time we have an almost continuous series illustrating the successive faunas, and in the interglacial beds they have been much studied. I shall now take the evidence that these last afford us into consideration, and that nothing may be overlooked I shall take my examples from the "Great Ice Age" of Mr. James Geikie, who is one of Mr. Croll's most ardent supporters. First of all, we may dismiss all the Scotch interglacial beds as negatively hostile to the theory, as they either contain no organisms at all, or—in a few cases—some shells of arctic types; nowhere have more southern forms been found than those existing off the present coasts. Coming to England, we have the marine shells of the west coast interglacial beds,—those found on Moel Twyfaen, at Macclesfield, and generally over South Lancashire. I have, in another place, argued that these shells are of older date than the Glacial period, and that they were pushed up out of the bed of the Irish Sea by the great glacier that filled it;* but I need not go into this argument here, as, whatever the evidence may be worth, it is again hostile, and Mr. Geikie admits that "upon the whole the fossils indicate colder conditions than now obtain in the Irish Sea."† On the eastern coast most of the shells that have been found indicate a colder climate, but at Holderness a few fragments of more southern species have been discovered. Messrs. Wood and Harmer, who have described these deposits, admit that they have been transported from some other area; and Mr. Croll has himself, with great acumen, shown how they might have been pushed up out of the German Ocean by the ice that brought over blocks of stone from Scandinavia and thrust them up on the same coast. However, whether brought by currents of water, as suggested by Messrs. Wood and

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* Nature, vol. x., pp. 25 and 62.

† Great Ice Age, p. 362.

Harmer, or by ice, as suggested by Mr. Croll, these broken and fragmentary shells—mixed through other transported material evidently ice-borne—are the *débris* of beds older than those in which they are now found. The Foraminiferæ of the same deposits have been examined by Messrs. Crosskey and Robertson: they, like the shells, are much worn, and present a more arctic character, varied by the presence of one or two Tertiary forms.* Altogether it appears that the deposits have been formed by the mixing together of the shells of two or more periods; and we might just as readily infer an arctic climate from the arctic shells and Foraminiferæ as a more southern one from the few fragments of species characteristic of the coralline crag, and which were probably derived from beds of that age in the neighbourhood.

In Ireland the shells found in the drift also indicate a colder climate than the present, and in Scandinavia the only evidence of the warm periods of Mr. Croll's theory, advanced by Mr. Geikie, points in reality to the opposite conclusion; that is to say, beds in Scania, described by M. Nathorst, containing Arctic plants—amongst others *Salix Polaris*, now confined to the Arctic Circle,—which indicate a climate more severe than that of Northern Norway. These and other beds so far north are valuable, as evidence that the ice did not destroy all remains of the vegetation that had flourished in the so-called "inter-glacial period," and if during that time more southern forms had ever advanced northwards we ought somewhere to find their remains.

In North America there is, again, no evidence of a warmer climate having prevailed in inter-glacial times; the marine shells and the vegetable remains all point either to more Arctic conditions, or to a climate not warmer than the present.

But, even if we could bring ourselves to believe that all the remains of the southern faunas and floras had been destroyed by the ice of the Glacial period, whilst the more Arctic forms had been preserved, we ought surely to find some evidence of the warm climates to the south of the limit to which the ice extended. In Sicily are preserved abundance of memorials of the cold climate of the Glacial period, when Alpine ice filled all the lakes of North Italy, covered the plains of Piedmont and Lombardy, and cooled the waters of the Mediterranean so that it was occupied by more northern species of mollusca, such as *Cyprina islandica* and many others. In Southern Sicily a magnificent series of shells have been preserved in rocks rising 2000 feet above the sea. Amongst the latest of these deposits, the northern forms of mollusca appear, and they are nowhere accompanied, followed, or immediately preceded by these tropical species that we ought to find if Mr. Croll's theory be true; to obtain them we must go back to Early Tertiary times, to the Miocene and Eocene periods. These alternations of climate cannot have taken place; it is not possible that all memorials of Arctic faunas and floras in the Eocene and Miocene periods, and all the remains of tropical species in the Glacial period could have been destroyed, whilst in the former case the southern forms, and in the latter the northern, were abundantly preserved. And yet, strangely enough, we are told by the advocates of this theory that it is in harmony with geological facts.

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* Introduction to Crag Mollusca. By S. V. Wood, Jun., and F. W. Harmer. P. 22.

Coming down to post-glacial times, we have in the marine shells only evidence of a gradual amelioration of the climate. Some of the freshwater beds are, however, supposed to indicate that, immediately after the Glacial period, a warmer climate prevailed than we enjoy at present. They only, however, show that it was a more Continental one, which is in accordance with other facts indicating that the British Isles were then joined to Europe by continuous land. I have published my reasons* for believing that a great river, into which flowed the waters of the Rhine, the Thames, the Seine, and many other streams, ran southwards, through what are now the Straits of Dover and the English Channel, as far as, and possibly further than, the Bay of Biscay, at a time when the level of the sea stood much lower than at present. The ice of the Glacial period had then retired from the southern portion of the bed of the German Ocean, but the flow of the waters northwards was still stopped by it, or, as I now think more probable, by the great moraines left across the ocean bed by it. Mr. Godwin-Austen has, in his various classical papers on the post-tertiary beds of the British Channel, shown the great probability that the Straits of Dover did not exist until after the Glacial period, but that a neck or isthmus of land stretched across, joined England to the Continent, and divided the German Ocean from the English Channel. Now, at the height of the Glacial period, we know that the greater part of the bed of the German Ocean was filled with ice, that stretched from Scandinavia to the coasts of Norfolk, if it did not extend further south. At this time the southern part of the German Ocean bed must have been occupied by a great freshwater lake whose arms ran up the valleys of the Thames and other rivers. The commencement of the cutting out of the Straits of Dover was, I believe, caused by the overflow from this great lake finding an outlet across the neck of land, which was gradually worn down, and the beds of gravel mantling all the lower hills of the Thames valley were, I think, beaches formed at the successively lower levels at which the lake stood. The ice to the north was now gradually receding, and leaving great banks of moraine rubbish in the old ocean bed, to be ultimately levelled by the sea when it long afterwards returned, and which now form the Dogger and other great submarine banks. At the highest point at which the freshwater lake stood, and which marks the extreme rigour of the Glacial period, we have no organic remains, but many boulders in the beach deposits apparently transported by coast ice. Lower down, the ice had retired a little to the north; the climate was still severe, but the mammoth, the woolly rhinoceros, the musk-ox, the lemming, and other animals fitted to live in an Arctic climate, left their remains in the old beaches. Still lower, we find more southern mammalia coming upon the scene, accompanied by freshwater shells, three of which are not found so far north. I thought formerly that their presence merely intimated a lowering of the lake in autumn, or that the ice had melted so far back that it partly drained around Scotland; but, on fuller consideration, I cannot believe that the hippopotamus came up, or the *Cyrena fluminalis* permanently lived in, water chilled by the melting of Continental ice; and I have come to the conclusion that the ice must have retired so far back that it drained entirely to the north of Scotland, and that it had left a great moraine stretching across the ocean bed, where the Doggerbank now lies, that closed the flow of the southern waters northwards. The Straits of Dover, and probably another barrier much further to the west, had by this time been so far cut through that the rivers stood but little above their present levels when the hippopotamus came up them, possibly only in summer. Then, too, existed great river conditions similar to those under which *Cyrena fluminalis* now thrives in Cashmere and Africa. As some additional evidence in favour of this theory, I may add that one of the three river-shells, *Unio pictorum*, has been dredged off the mouth of the British Channel in the course of the supposed great river.

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* Nature, vol. x., p. 25.

On the continent of Europe, and in North and South America, no evidence whatever has been found to indicate a sub-tropical climate having prevailed in post-glacial times in the temperate regions of the globe, and I cannot but consider that the issue that Mr. Croll has based on the existence of warm climates having existed about the same time as, and intercalated between, his cold climates, must be given against him. If so, it is fatal to his theory, for he has not one whit exaggerated the importance of the necessity of these oscillations of temperature. If the theory be true, each hemisphere endured the extremes of heat and cold. Just as much as the Glacial period lowered the temperature of any place below what it is now, so must the warm period that came on in about ten thousand years have raised it, and it is a rigorous deduction from the theory that, either in the southern or the northern hemisphere, or both, there must have intervened a great period of warmth as great as that of the Miocene epoch since the countries were glaciated.

There are some other facts to be accounted for that are not, I think, explained by Mr. Croll's theory, but they will be better understood if I take them into consideration under the next theory to be discussed.

3. *Theory of a Change in the Obliquity of the Ecliptic.*—So long ago as 1688, Dr. Robert Hooke drew attention to the evidences of tropical climates having prevailed in Europe, and speculated on changes in the axis of the earth's rotation, or a shifting of the earth's centre of gravity, or a change in the obliquity of the ecliptic. The last theory was a favourite one amongst the older English geologists, but even in these early days received little favour from astronomers, for Newton pronounced against it and declared that astronomy did not countenance the theory that there had been any change in the direction of the earth's axis. The celebrated Laplace investigated the problem of the effect of the attraction of the sun, the moon, and the planets upon the equatorial protuberance, and came to the conclusion that this could only cause a variation in the obliquity to the amount of 1° 21′. More recently, Leverrier has examined the same question, and has arrived at the result that it might vary to the amount of 4° 52′, but not more. The difference between Laplace and Leverrier is a large one, but most geologists have accepted their verdict as decisive, that former great changes of climate could not have been caused by variations in the obliquity of the ecliptic.

But granted that the great geometricians could not have erred very much in their calculations, we may still, without presumption, enquire whether there are not other elements of disturbance besides those they investigated. They assumed in their examination of the problem that the thickening of matter around the equator was a constant quantity, whereas there are evidences of great upheaval and depression in remote ages that may have altered the conditions of the question. The gradual heaping up of ice around the poles in the glacial period must have in some measure diminished the difference between the polar and the equatorial diameters. Many physicists believe that even now an elevation of land around the poles and a depression of land in the tropics is taking place.

The protuberance around the equator is not a regular one, but the equatorial circumference approaches in general outline to an ellipse, of which the greater diameter is two miles longer than the other. At the time the above-mentioned calculations were made the data did not exist for determining the irregularity. To the non-astronomical mind it appears evident that this great difference in the equatorial diameters is an element of great importance in the calculations, and as it was not considered we cannot admit that the problem has yet been decided. The great preponderance of land in one hemisphere, not arranged around the pole of the earth but in a mass whose centre is situated near the English Channel, must also be a disturbing element of no mean importance.

Our knowledge of the other planets teaches us that there is no limit to the obliquity of their axes. In Jupiter the axis is nearly perpendicular to its orbit, so that there is no change in the length of its day. In Saturn the obliquity is 29°, in Mars 30¼°, and in Venus it reaches the extreme amount of 75°, so that its tropics overlap considerably its arctic circle, and there are no temperate zones. The original cause of the inclination of the axes of the planets has never been demonstrated, and until this be done it may be allowable to suppose that changes may occur through the same cause.

Lieut.-Col. Drayson has approached this question in a different manner.* Leaving out altogether the consideration of the cause, he contends that a variation of the obliquity is taking place. He shows that according to the observations of the last four hundred years the obliquity of the ecliptic has decreased, and argues that the pole of the earth instead of describing a circle around the pole of the ecliptic describes a larger one around a point six degrees from that centre. It is admitted, and is indeed an established fact, that the obliquity is less than it was some centuries ago, but the generality of astronomers are agreed that this is owing to the small variation that the calculations of Laplace and Leverrier showed to be possible, and that it is simply a coincidence that the path described by the pole is that of a larger circle around a point a little distant from the pole of the ecliptic. They contend that the pole of the earth does describe a circle around the pole of the ecliptic as a centre, but that the outline of that circle is a waved one, and that during the time that observations have been made the direction of the pole has been down towards the trough of one of these waves, but that it will again rise as much above as it dips below the mean distance from the centre. It is an objection to this theory as well as to that of Lieut.-Col. Drayson that it is assumed that the pole of the earth describes a circle, whereas amongst the heavenly bodies we have no circular movements. All the orbits are ellipses of varying eccentricity, and from analogy we should be led to expect that the pole of the earth would not describe an exact circle. That it does so is entirely theoretical, founded on calculations based on the assumption that the earth's equatorial circumference is a circle, which it is not. Lieut.-Col. Drayson has informed me that though he has assumed the curve to be that of a circle, the earlier observations cannot be sufficiently relied on, and it may be that of an ellipse or of a spiral.

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* **The Last Glacial Epoch. Chapman and Hall.**

Until astronomers have re-considered this question with the light of our present knowledge of the figure of the earth, geologists should not be prevented from speculating on the possibility of great changes in the obliquity of the ecliptic having caused former great variations of temperature. According to Lieut.-Col. Drayson, the obliquity of the ecliptic has been as much as 35½°. The effect of this was, he urges, the production of the Glacial period. He states that as the arctic circle would then reach nearly to latitude 54½°, there would be an accumulation of snow during the winter; which during the summer, in consequence of the great altitude of the sun, would be melted nearly to the poles, occasioning enormous floods. Now if this really would be the effect of a greater obliquity of the ecliptic, we might at once dismiss it as a possible cause of the accumulation of ice in the glacial period, for it is evident that the great mass of ice—some thousands of feet thick—that moved down southwards over the northern parts of America, Europe, and Asia, could not have been the production of a single winter. It is possible that this and some other geological speculations of the author have prevented many from taking a favourable view of his theory, and it is of importance to discuss what would be the real effect of a greater obliquity of the ecliptic.

We are able to approach this question provided with data derived from the effects of the present inclination of the axis of the earth to the plane of its orbit. To it is due the varying length of the day throughout the year in the temperate and arctic zones, and the consequent production of the seasons. If the axis, as in Jupiter, were perpendicular to the plane of the orbit, night and day throughout the world would be equal. Every day there would be twelve hours' light and twelve hours' darkness. Each place would have but one season, and eternal spring would reign around the arctic circle. Under such circumstances the piling up of snow, or even its production at the sea-level, would be impossible, excepting perhaps in the immediate neighbourhood of the poles, where the rays of the sun would have but little heating power from its small altitude.

Our summer and winter are therefore due to the present obliquity of the ecliptic, and so also is it that now around the poles some lands are being glaciated, for excepting for that obliquity snow and ice could not accumulate, excepting on mountain chains. The obliquity of the ecliptic does not affect the mean amount of heat received at any one point from the sun, but it causes the heat and the cold to predominate at different seasons of the year. Near the poles there are six months' night and six months' day, but the absolute amount of heat that arrives from the sun is the same as if there were twelve hours' light and twelve hours' darkness every day. The cause of perpetual ice and snow is not, as I have already shown, the cooling of the air by the melting snow in summer, nor the formation of clouds shutting off the rays of the sun. It is, I believe, in consequence of the reflection into space of many of the rays of light and heat that fall on a snow-covered surface, and any cause that tends to increase the amount of snow or to extend the snow-covered area will tend to chill the climate of such parts by occasioning more of the rays of the sun to be deflected and lost. Therefore a long hot summer and a long cold winter are more likely to favour the accumulation of perpetual snow than a place under exactly the same conditions, where a thermometer exposed to the rays of the sun would register the same amount of heat received, but where the sun rose and set every twelve hours, so that the heat by day and the cold by night were never so excessive.

Thus, if we suppose the earth's axis to have been originally perpendicular to the plane of its orbit, so that it had twelve hours' night and twelve hours' day all over the world, and that from some cause or other the axis began to incline and the inclination gradually to increase, the seasons of the year in the temperate and arctic zones would tend to become more and more distinct. An ever-widening circle around the poles would be covered by snow during the cold winter, and lower the temperature of the summer by reflecting the rays of the sun as long as it lasted; and if the obliquity increased to a greater amount than at present, so would a greater area be brought under arctic conditions, and an approach be made to the cold of the Glacial period.

The accumulation of snow is dependent on another factor, namely, increased precipitation; and I doubt if any theory would satisfy the conditions of the case that simply increased the cold of the glaciated regions without providing for an increased evaporation outside these regions, and thus to allow greater precipitation upon them. An increase of the obliquity of the ecliptic satisfies this condition, for whilst on one hand the arctic circle would be extended, so on the other would the tropics; one part of the temperate zones, that next the poles, would have its mean temperature greatly lowered; whilst the other, that nearest the equator, would have its temperature raised and become an evaporating area. Thus, supposing Lieut.-Col. Drayson to be right in his theory, that at one time the obliquity was as much as about 35½°, the arctic circle would then reach to latitude 54½°, and the tropics to 35½°, reducing the temperate zones from their present width of 43° each to only 19°, one-half of the decrease being added to the arctic circle and one-half to the tropics. As soon, also, as the ice had extended so far as to shut off the warm currents of the ocean that penetrate nearly to the pole, much of the heat now spent in melting the ice of the arctic circle would be expended in evaporation, and precipitation would be proportionally increased.

Those who have followed me in this short argument will, I believe, admit that an increase of the obliquity of the ecliptic does appear to be sufficient to cause an addition to the snow and ice piled up around the poles; and we may now inquire if the theory throws any light on other problems of the Glacial period, and is in harmony with the facts of geology. In doing this I shall contrast it with the other cosmical theory. The theory of the greater eccentricity of the orbit requires that the glacial periods of the two hemispheres should be at different times; that of the greater obliquity of the ecliptic, that they should be simultaneous. There is not much evidence available, but what little there is, is in favour of the glaciation of the two hemispheres having occurred at the same time. Thus, there exist glacial conditions at present around the poles, due primarily to the obliquity of the ecliptic, and these conditions are contemporaneous in the two hemispheres. More ice and snow is heaped up within the antarctic circle than at its antipodes, because a greater evaporating area of ocean surrounds it, whilst the arctic regions are almost circled by land that not only lessens the evaporating surface, but intercepts much of the moisture-bearing currents from the south. The snow piled up on the Himalayas, the Alps, and other high northern ranges, is just so much prevented from reaching the arctic regions. That the difference is due to lessened precipitation, and not to a difference of temperature, will be seen if we follow the isotherm of 30° around each hemisphere. We shall find it deviating but little in the southern hemisphere from the line of lat. 60°, being now a little to the north and now a little to the south of it. In the northern hemisphere the isotherm of 30° is much more irregular, sometimes running far to the south, sometimes far to the north, but the mean is again about lat. 60°, proving that if there was as much precipitation there would be as much ice and snow to the north of lat. 60° N. as there is to the south of lat. 60° S. Even now, if all the snow of northern mountain ranges was added to that existing to the north of lat. 60° N., the difference would be greatly lessened, and we should have in both hemispheres a partial Glacial period reaching nearly 30° from the poles, and produced by the present obliquity of the ecliptic. Only on one of the other planets has an accumulation of snow at the poles been proved to exist, namely, on Mars; which, with an obliquity of 30¼°, is glaciated at both poles at the same time. So that, judging from analogy, we might expect the glacial period of the two hemispheres to have been contemporaneous.

Many plants and some animals are found, in both the northern and southern temperate zones, separated by the whole width of the tropics, which they cannot now pass;

and Mr. Darwin has explained their presence by supposing that during the glacial period they were driven to the high lands of the tropics by the advancing ice, and that on its retreat they followed it north and south. A glacial period in one hemisphere only would not afford this means of migration; the plants and animals driven south by the northern ice would always have a hot zone to the south of them, which they could not pass.

Another class of evidence that favours the theory of the glacial periods of the two hemispheres having existed at the same time, is that connected with the lowering of the sea-level. Mr. Alfred Tylor, some time ago, advanced the theory that the piling up of ice in the northern hemisphere would lower the level of the ocean 600 feet. Mr. Croll has lately discussed the question,* and comes to the conclusion that, if each hemisphere was glaciated alternately, the level of the ocean would be raised, and not lowered, in the one in which the ice accumulated; by the melting of the ice of the opposite pole and the shifting of the centre of the earth's gravity towards that covered by an ice-cap. Though I cannot agree with Mr. Croll's estimate of the thickness of the ice, and think that it could not possibly have been highest at the pole, I have no doubt that a great lowering of the level of the ocean could not have arisen by the accumulation of ice at one pole, if at the same time that now existing at the other was melted off. But if the glacial periods of the two hemispheres were simultaneous, the water abstracted from the sea and frozen into ice at the two poles, and that impounded in the great lakes of Northern Europe, America, and Asia, by the blockage of the northern drainage of the continents by ice, must have lowered the level of the ocean to a great extent.

In my "Naturalist in Nicaragua" I stated that this decrease in the volume of the ocean could not have been less than 1000 feet. I was thus guarded because we had at that time no proof of the ice having descended from the north upon Northern Asia, and there was no certainty that the Polar basin had been filled with it. Since then I have myself found evidence in Siberia that the Arctic Sea was filled with ice, which was piled up so high that it overflowed the low lands as far as lat. 52° N. Calculating from this data, I find that the lowering of the sea-level—on the supposition that the ice was equal in the two hemispheres at the same time—could not have been less than 2000 feet, and may have been much more. A glacial period in one hemisphere only would not produce this result, and therefore any evidence that tends to prove that the level of the ocean was greatly lowered in the glacial period is also evidence in favour of the northern and southern ice having been contemporaneous.

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* Geological Magazine, July and August, 1874.

Over the whole world the distribution of many insular faunas and floras has been explained by the supposition that the islands were at one time joined to continents near them and to each other, in post-tertiary times. In every case that I have examined, the theory is that the last movement of the land has been one of depression. Thus the land over which the flora of Greenland reached that country from Europe is supposed to have sunk down. The lands connecting England with Ireland and the Continent, during the forest periods before and after the culmination of the glacial epoch; the land connecting Malta with Africa; that joining the Islands of the Malay Archipelago on one side to Asia, on the other to Australia; that connecting the West-Indian Islands with Venezuela and Yucatan; and that uniting Tasmania with Australia,—are all supposed to have been submerged by a sinking of the land, and we have in the same areas no corresponding instances of elevation. Whilst all islands having shallow channels, however broad, separating them from each other and from not distant continents, produce evidence of having been formerly connected in post-tertiary times, on the other hand islands surrounded by deep water are distinguished by peculiar faunas. Thus Madagascar is separated from Africa by a deep sea, and its fauna is wonderfully distinct, though it still shows traces of a geologically remote connection with that continent. The Gallapagos Islands are a still stronger case, for though near together they are separated by channels of great depth, and Darwin found them tenanted by distinct species of reptiles, birds, and plants. If the channels were made dry land, by the lowering of the sea, we easily understand why islands surrounded by deep water did not lose their insular character; but on the supposition that they have been produced by movements of the land, the reason is not obvious why the depressions should have been limited to a certain depth.

All round the British and Irish coasts, and around Western Europe, we have submerged forests passing under the bed of the ocean. Some—as that at Cromer—are older, others newer, than the greatest development of the ice of the glacial period. To allow these forests to have grown, we have to suppose an elevation, and for their submergence a depression, of the land,—on the theory that it was movements of the earth's crust that brought it above and sank it below the sea. Now, in various places in the south of England, we have marine deposits a little older than the forest bed of Cromer: they occur mostly between the present tide-marks,—never higher than we may suppose the tide to have reached before the Straits of Dover were cut through. Therefore, if the surface of the land has oscillated, it is remarkable that it should have returned to the same level as it stood at before the Glacial period; but such a fact is clearly in unison with the idea that it was the mobile water that had retreated and returned. These submerged forests are not confined to Europe, but are found on the coasts of America,—as in the Bay of Fundy,—betokening that their occurrence belongs to a general and not to a local cause.

Another class of phenomena, usually ascribed to a gradual sinking of the earth's crust, but which might also be produced by the return of the sea to the level it stood at before the Glacial period, is that connected with the growth of coral islands. Darwin's celebrated essay on their formation first proved that they were due to the gradual deepening of the water. Dana, closely following Darwin in his theory, estimates that this deepening of the ocean bed from which the coral islands rise has been at least 3000 feet, and that the subsidence to which he ascribes it extends round one-fourth of the earth's circumference in the Pacific, being indicated by atolls in that ocean for 6000 miles in length and 2000 in width. In the Atlantic he considers that "the Bahamas show by their form and position that they cover a submerged land of large area, stretching over 600 miles from N.W. to S.E. The long line of reefs and the Florida Keys trending away from the land of Southern Florida are evidence that the Florida region participated in the downward movement."*

Nor are these indications of either a subsidence of the land or a rise of the level of the ocean since the Glacial period yet exhausted. C. F. Hartt considers he has found proofs in Brazil that that country stood higher when it was glaciated than it now does.† Dana has argued the same respecting the high latitudes of North America. There is hardly a mountain chain of the world that has not been supposed to have stood higher, to account for the lowering of the snow-level on its sides in the Glacial period. The Himalayas, the Alps, the Caucasus, the Pyrenees, the mountains of Syria, the Atlas Chain, the mountains of New Zealand, of California and Central America, and many others, show distinct traces of glaciers having descended either on ranges where snow now never accumulates or even falls, or else thousands of feet below the present snow-line. It has by some been considered a simple explanation of these facts, to suppose that each mountain chain was elevated a few thousand feet in the glacial period, and has since sank down. Here the land went up and here it went down, they say, and think they have found a solution, without explaining why it should or how it could have done so. I shall have some more remarks to make on this assumed elasticity of the earth's surface, but now pass on to remark how a general lowering of the sea-level would cause the snow-line to descend on every mountain chain. Mr. H. W. Bates has pointed out to me, what seems perfectly obvious when once noticed, but what had certainly not occurred to me when I first wrote on this subject, namely, that a lowering of the sea-level would produce the same effect upon the climate of any place as a rise of the land to about the same amount as the atmosphere would sink with the sea. I find that Humboldt, in whose writings are found the germs of many later theories, had made the same observation.* I fail to see how glaciers could be produced in the tropics on mountain chains far below the present snow-line in the Glacial period if it was caused by an increase in the eccentricity of the orbit; for that could not affect the mean temperature of the tropics where day and night were equal, and the heaping up of ice at one pole could not lower the sea-level much; but if it was caused by an increase of the obliquity of the ecliptic, the mean temperature of the tropics would be lowered through the path of the sun being lengthened, the snow-line would descend still farther by the lowering of the sea, and still farther from increased precipitation, owing to the greater evaporation that would take place when the shallowing of the sea shut off cold currents from the polar regions. The combination of these factors could not fail to lower the snow-line in the tropics thousands of feet, as we find it to have been lowered in the Glacial period.

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* Coral Islands, 1872, p. 366.

† Geology and Physical Geography of Brazil. By C. F. HARTT. P. 573.

The examination of the deltas of the great rivers—the Mississippi, the Ganges, the Nile, and the Po—have shown that there are land-surfaces and freshwater deposits hundreds of feet below the level of the sea. All our English rivers run in old channels now filled up nearly to low-water line, but which are excavated in the solid rock for hundreds of feet below it. These all prove that either the land stood higher or the sea lower, and I cannot but agree with Mr. Alfred Tylor, who has ably discussed this question, that the cause is not a local one, but a general lowering of the level of the ocean all over the world in Glacial times.

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* **Edinburgh Philosophical Journal, vol. iv., p. 267.**

To these many evidences of a rise of the level of the sea produced by the melting of the ice of the Glacial period, I think I may fairly add the traditions of mankind of one or more great deluges that overwhelmed peopled lands. In America, Africa, and Asia the remembrance of great catastrophes that nearly exterminated mankind in certain regions has been handed down, indistinctly it is true, but with a marvellous resemblance in the traditions preserved in countries of the world far removed from each other. Here, again, I think that such a general explanation as that of the rise of the waters of the ocean submerging low-lying peopled countries—accompanied by earthquake convulsions, such as were likely to be occasioned by the strains on the earth's crust when the ice melted off the mountain tops and the polar regions, and ran down to the ocean beds—is a more likely theory than that the traditions refer to local catastrophes.

We have proofs that man existed even in England before the presumed date of the return of the waters of the ocean. When the great lake that I have mentioned filled the southern part of the bed of the German Ocean, whilst the northern part was still occupied by the retreating ice, man appears on the margin of that lake when it stood about two hundred feet above the present sea-level. He follows its receding shores as the great river running from it cuts through the barriers in the English Channel, and throughout its gravelly beaches his flint implements are found along with the bones of the great mammalia. The lake is gradually lowered until the rivers running into it stand only about twenty feet above their present level, and the hippopotamus and other southern mammalia now come up the great river occasionally; then palæolithic man, and the great mammalia on which he possibly subsisted, disappear together, and the waters of the sea occupies the bed of the German Ocean and the channel of the great river. Not from such rude tribes was, however, the story of the great deluge handed down; but during the Glacial period a belt of higher civilisation seems to have girdled the world on the borders of the northern tropic, and it was probably the remnants of ruined and engulphed kingdoms of that zone from which the traditions have come down.

Mr. A. G. Renshaw has pointed out to me that the melting of the ice of the Glacial period must have occupied thousands of years, and I am quite convinced that it must have done so. The gradual growth of coral islands, and the silting up of deltas filled with fresh-water deposits, cannot be explained if we adopt the hypothesis that the ice was suddenly melted. But we do not require thousands or even hundreds of feet of submergence to overwhelm low-lying tracts of country, and I think we may fairly assume that there would be some sudden rise of the sea-level, scores of feet at least, through the rapid melting of great quantities of ice, as, for instance, when the warm ocean currents from the south first gained access to the Arctic regions, or when the immense fresh-water lakes of northern Europe and Asia, pounded back by the ice, broke through their melting barriers and ran down to the ocean. Marine deposits found alternating with land surfaces in the deltas of the Mississippi and the Po indicate such occasional more rapid advances of the sea. It may be said that I am advancing one theory—that of the lowering of the sea-level during the Glacial period—to strengthen another—that of the production of the Glacial period by an increase of the obliquity of the ecliptic ; but the lowering of the sea is more than theoretical,—it is a necessary consequence of the heaping up of ice around both poles at once, and any evidence that it was greatly lowered in Glacial times is also evidence in favour of the theory of the increase of the obliquity of the ecliptic, which would produce a Glacial period in the two hemispheres at the same time.

Whilst we have thus many indications of a general rise of the sea-level since the culmination of the Glacial period, we have a remarkable exception in a rise of land towards the north and south poles, which is believed to be still in progress. In the southern hemisphere it is certainly still in operation at intervals in the southern extremity of South America and in New Zealand. In the northern hemisphere it has been better observed on account of the greater amount of land around the polar regions. One line of elevation commences in Scandinavia at Stockholm on the eastern, and near Gothenburg on the western, coast, increasing northwards as far as the North Cape, where there are marine post-glacial deposits 600 feet above the sea. The land has been elevated since the Glacial period, for the raised beaches everywhere rest on the boulder clay with transported blocks. Professor Kjerulf, of Christiania, has shown that the highest sea-terraces contain Arctic shells, which indicate that the movement of elevation had commenced when the waters were much colder than now.* This movement appears to be continued eastward round the Arctic sea, for, according to Wrangel, the land is slowly rising around the northern extremity of Siberia. He notices the occurrence of marine beds containing sea-shells of existing species along with the remains of the mammoth several feet above the sea-level. There is some evidence that the coasts of Scandinavia are still rising.

Another line of elevation runs north from near New York. At Brooklyn the sea-beaches with marine shells occur 100 feet above the sea. This elevation also increases northwards. At Quebec and Montreal it reaches between 400 and 500 feet, and much farther to the north within the arctic circle, on the shores of Barrow's Straits, it has carried up sea-shells of existing species to a height of 1000 feet above the ocean. The movement clearly increases towards the pole. How far it extends westward I do not know, but it decreases eastward from Montreal, and in Nova Scotia I could find no traces of any elevation having taken place.

Against these numerous instances of upheaval we have in northern regions the solitary instance of a depression of part of the coast of Greenland believed to be still in progress; and it is a very suggestive fact that that country is at present enduring intense glaciation and buried in snow and ice piled up mountains high upon it. Seeing, then, that towards both poles, with a single exception, there has been a rise of land, in some parts still going on, in all evidently accomplished since the Glacial period, it is an important enquiry whether the land so raised was above or below the level of the sea in pre-glacial times. Within the arctic circle the evidence is clear that it was not, for nowhere have Tertiary marine shells been found. Nor can it be argued that they may have existed, but have been destroyed by the ice of the Glacial period, for Tertiary land-surfaces and land-plants are abundant and well preserved. This points to the conclusion that like Greenland at present the land around the poles sank down after it was covered by ice, and has been slowly rising since it melted away. It is a legitimate speculation, and one fully warranted by the facts of the case, that the cause of the depression was the piling up of a vast weight of ice around the poles, and the cause of the elevation the removal of that vast weight by the melting of the ice. That the movement of elevation continues in some places only shows that the earth is a rigid body and but slowly gives way to great strains. We must, according to Mr. Croll's theory, go back 200,000 years for the height of the Glacial period; but not much more than one-tenth of that time would be sufficient according to the theory of an increase in the obliquity of the ecliptic; and I submit that the shorter interval is more in accordance with the continuance of the polar movements, the facts connected with the progress of civilisation northwards, and the little change there has been in the fauna and flora of the world since the Glacial period.

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* The Terraces of Norway. Translated from the Norsk by Dr. MARSHALL HALL.

If our Glacial period was merely the heaping up of ice and snow around the North Pole that now exists on both hemispheres, the result would only be a slight shifting of the centre of gravity of the earth northwards; but if it was contemporaneous in the two hemispheres, as would result from a greater obliquity of the ecliptic, the figure of the earth would be changed, its polar diameter would be lengthened, its mean equatorial diameter shortened, and a series of strains would be set up tending to restore its figure of equilibrium. And if during the Glacial period the shape of the earth had approached that of equilibrium through the sinking down of the land around the poles and the rising of land in the tropics, then, when the ice melted away, the polar diameter would be shortened, the mean equatorial diameter lengthened, and forces would be set in operation, tending to lower the land of the tropics, and raise that around the poles. Therefore I am ready to admit that some part of the deepening of tropical oceans as evidenced by the growth of coral islands and reefs—and especially any now going on—may be due to a sinking of the bed of the ocean; but in doing so I by no means admit that the whole or even the greater part of the 3000 feet or more of depression that has taken place, according to Dana, can be ascribed to that movement. But the whole of the deepening of the sea, both that arising from its surface being raised, and that by portions of its bed being depressed, has, I believe, been caused by the gradual melting of the ice of the Glacial period, liberating the water that had been piled up at the poles, and disturbing the equilibrium of the figure of the earth.

I know that eminent physicists ascribe the movements of the earth's surface to its contraction from secular cooling, and Mr. Mallet has lately ably argued that volcanoes are one of the results of the movement due to that contraction. Without wishing to call in question any theories about the earth having once been in a state of fusion, I can find nothing to warrant the conclusion that for long geological ages it has cooled in any appreciable degree. Laplace, reasoning from astronomical observations made in the time of Hipparchus, calculated that during the last 2000 years there has been no appreciable contraction of the earth by cooling, for the length of the day has not been sensibly shortened, not even to the amount of 1-300th of a second, so that the contraction of the globe must have been inappreciably small or none at all, as it could not take place without affecting the length of the day. We may therefore ask how an amount of contraction inappreciable in 2000 years can have resulted in the great amount of movement of the earth's crust and the vast volcanic energy now apparent, or why should its tendency be to lengthen the polar and shorten the equatorial diameters ? and are not such movements more in accordance with the cause I have suggested ?

It is true that the earth must radiate heat into space ; but it is not evident that it radiates more annually than it receives from the sun, and if it does not it is not a cooling globe. If earthquakes and volcanoes are the result of movements of the earth's crust—produced, not by contraction, but by the strains set in action by the melting of the ice caps of the Glacial period—so probably is what we call the internal heat of the earth increasing in depth in our mines. The usually accepted theory that the increased temperature in depth is due to a greatly heated or even fluid fused nucleus, slowly giving off its heat towards the surface, does not explain the irregular distribution of the heat. To my mind it is much more conceivable and more probable that the centre of the earth is as cold as space, and that the movements of its upper strata and the heat they give rise to are confined to a comparatively shallow envelope, say not more than 500 miles thick.

The insufficiency of the theory of central heat was strongly impressed upon me when I was studying the facts connected with the frozen soil of Northern Siberia. At Yakutsk the soil—excepting a few feet at the surface which is thawed every summer—is permanently frozen to a depth of about 400 feet. This frozen soil extends to the shores of the Arctic Sea, and in many places the cliffs bordering the rivers are composed of alternate layers of soil and ice. It is in these cliffs that the bodies of the Arctic rhinoceros and mammoth have been found with their flesh still preserved. As Lyell as remarked, since they were entombed, the soil cannot have thawed for a single season or their flesh would have putrefied. The ice, therefore, is as old as the close of the Glacial period, at which time these great quadrupeds flourished, and at Yakutsk has remained unmelted all that time. It seems impossible that it could have done so to a depth of 400 feet from the surface if the earth was a cooling globe. If, however, the heat of the crust of the earth is due to movements within it, we can understand that in Siberia it may not have been developed to the same extent as in other parts; for, according to the researches of Murchison, that country is situated on an area of great geological stability. According to Von Cotta, it was never below the level of the ocean from the close of the Permian epoch up to the Glacial period; and I have been able to determine that this permanence of level has continued up to the present time, and that the strata of the Steppes are fresh-water deposits, excepting those round the extreme northern extremity of the country.

If, whilst accepting Mallet's ably worked out theory that volcanoes are the result of movements of the crust of the earth, I am right in ascribing these movements—not with him to the secular cooling of the globe—but to the forces tending to restore the equilibrium of the earth's figure, disturbed by the accretion of ice at the poles during the Glacial period and its subsequent liquefaction, it will add another to the many wonderful effects due directly and indirectly to the action of the sun. It was the heat of the sun that raised the water by vapourisation to the level at which it congealed near the poles; and after the earth had approached its normal form by the sinking of polar lands, it was the heat of the sun that disturbed the equilibrium again by melting the snow and ice and allowing it to flow towards the equator. Not only volcanoes but the folding of strata might be produced by these movements of elevation and depression; but I guard myself against expressing an opinion whether or not the earlier and greater geological folds and upheavals might not be due to other causes.

I have now brought forward a great variety of evidence, drawn from very different sources, that points to the probability of the Glacial periods of the two hemispheres having been contemporaneous. Of the two astronomical theories it is in favour of the one founded on a great increase in the obliquity of the ecliptic, for that would cause a heaping up of ice around the two poles at the same time. I shall now turn to the consideration of a most important class of facts only incidentally alluded to in the foregoing pages.

We have not only to account for the cold of the Glacial period, but for its converse—the heat of Early Tertiary times.

The same latitudes that in the era of greatest cold were covered with continental ice, or bore just beyond the reach of the great glaciers the stunted Polar willow and a few Arctic mosses and lichens, where the musk-ox and the Greenland lemming found their northern limit in summer, were at the commencement of the Tertiary period covered with subtropical forests. Palm-trees—of types now restricted to the Moluccas, the Phillippine islands, and Bengal,—with custard-apples, melons, and many another tropical and subtropical plant, flourished in the neighbourhood of Paris and London. Huge animals, resembling but larger than tapirs, roamed in these forests; monkies chattered amongst the trees; great tortoises crawled beneath the rank herbage; sea-snakes, crocodiles, and enormous sharks tenanted the waters. If any of the mammalia had at that time become adapted to live in an Arctic climate, they must have retired to the very Pole to find it.

It is these two extremes of heat and cold with which we have to deal. If we confine ourselves to the attempt of accounting for the cold of the Glacial period alone, we grapple with but half the problem. The climate of the Eocene period was apparently as much warmer as that of the great ice age was colder than the present. The converse of the cause of the one extreme in all probability produced the other. It has been my fortune in other branches of enquiry to find in one hemisphere the solution of a question that had puzzled me in another. For instance, the origin of large masses of gold in the gravel-beds of Australia, in districts where the auriferous lodes contained only fine grains of gold, remained doubtful until I found nearly at the antipodes of the first observation, in Nova Scotia, that the very highest parts of the lodes had in some cases been left undenuded, and that there the gold occurred in large pieces, whilst deeper only fine grains were found. The conclusion was obvious that in Australia the tops of the quartz veins containing the "nuggets" had been worn off and carried down into the valleys. And so, in studying the Glacial question, it was not until I occupied myself with the consideration of its antipodes, the climate of Early Tertiary times, that I laid hold of facts that left in my mind little doubt as to what had been the prime cause of the great oscillations of temperature.

When the subtropical fauna and flora lived in Central Europe as far as 52° N. lat., still nearer the Pole vegetation flourished similar to that which now characterises the milder portions of the temperate zones, and the representatives of the present flora of Northern Europe lived and throve within about 11° of the Pole. Thus at Spitzbergen, far within the Arctic Circle, in lat. 78° 56′, flourished species of hazel, plane, poplar, lime, and beech trees, and Professor Heer considers that firs and poplars must at that time have reached to the North Pole if there was land there then for them to grow on.

The strata in which this fossil flora is found within the Arctic Circle are believed by geologists to be of Miocene age. This determination is based on the fact that of 137 species of plants found in the Greenland beds, 46, or one-third, are identical with species of the Miocene flora of Central Europe. This fact, however, seems to me rather to be in favour of the different age of the two deposits. It is improbable that so many species should have had such a wide range. We have seen that in the Eocene period Central Europe was occupied by a subtropical fauna and flora. Is it not likely that the time of the greatest heat in Europe was also the time of greatest heat within the Arctic Circle, and that, on the advent of the cooler climate of the Miocene, some of the species that had lived much further north migrated southwards into Central Europe, and took the place of the Eocene flora, for which the climate had then become too cool? In correlating the age of the arctic flora with that of the Miocene of Central Europe, we may be making the same mistake as future geologists would do if they assumed that the beds lying above the Cromer forest lived at the same time as some arctic ones now forming because they contain the same plants, yet the former beds were deposited at the very commencement of the Glacial period. I am not sure that the omission of the consideration of the important part that the varying climates of the Tertiary period played in causing the faunas and floras to migrate from one latitude to another may not have led to an exaggerated opinion of the great length of time occupied in forming the strata. It matters not, however, for my argument whether the arctic flora, of which we have such abundant remains, was of Miocene or Eocene age. What I have to say has nothing to do with the existence of the same species at the same time in Central Europe and in North Greenland, but with the fact that such plants were able to live at all so far north. To avoid any mistake I prefer, however, to speak of them as Early Tertiary.

In a paper on the Miocene flora of North Greenland read before the British Association in 1866, Professor Heer mentioned that more than sixty different species of plants brought from Atanekerdluk, North Greenland, situated in lat. 70° N., had been examined by him. Amongst the trees, the most abundant is the *Sequoia Langsdorfii*, the nearest living ally of which is the *Sequoia sempervirens*, not now found farther north than lat. 53°, and which requires a mean annual temperature of at least 49° F., and that in winter the thermometer should not fall below 34° F. Cones of a magnolia have been found, proving, as Lyell has remarked, that this splendid tree not only lived, but ripened its fruit within the Arctic Circle. Vines also "twined round the forest trees, and broad-leaved ferns grew beneath their shade."* Some of the trunks of trees observed were thicker than a man's body, and one seen by Captain Inglefield stood upright as it had grown.

Nor, as we have seen, did this Early Tertiary flora end in Greenland, but is found, containing a large number of species of trees, in Spitzbergen, in lat. 78° 56′ N., or about 11° from the Pole. Prof. Heer considers that the winter temperature in Greenland could never have fallen below 34° F.; and says—"These conclusions are only links in the grand chain of evidence obtained from the examination of the Miocene flora of the whole of Europe. They prove to us that we could not, by any re-arrangement of the relative positions of land and water, produce for the northern hemisphere a climate which would explain the phenomena in a satisfactory way. We must admit that we are face to face with a problem whose solution in all probability must be attempted, and we doubt not, completed by the astronomer."

Whilst there are many reasons, as I have shown in the first part of this paper, for believing that the mean temperature of England might be greatly reduced by geographical changes, there is nothing whatever to lead us to conclude that the present mean temperature of Spitzbergen could be raised by any alteration of the relative positions of the sea and the land. By the present arrangement a large body of warm water is poured past that island, deflecting the isothermal lines in a great tongue northwards, which embraces it in its apex; and no conceivable geographical re-arrangement could raise its mean temperature more above that due to its latitude than what is effected at present.

No re-distribution of land and water could compensate for the length of the Arctic night. Lyell has speculated on the possibility of the trees living without light for months, and thinks they might survive through the long darkness.

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* Student's Elements of Geology, p. 223.

But long nights mean extreme cold: the one cannot occur without the other. The earth rapidly radiates its surface-heat into space, and, if the loss be not compensated for by what is received from the sun, the temperature soon falls far below the freezing-point.

Neither could any possible increase in the eccentricity of the earth's orbit alter materially the length of the Arctic night; nor could the moderate amount of change allowed by astronomers in the obliquity of the ecliptic. Taking their highest limit, the Arctic night in lat. 78° 56′ would still last for three months, during which the sun would not rise above the horizon. It is impossible but that the radiation from the earth during that time would produce intense cold. This long night could be lessened in one way, and one way only,—by a much greater change in the obliquity of the ecliptic than astronomers have yet admitted can have taken place.

In an enquiry of this kind it is well when we can get down to such a crucial fact as is that of the flourishing of many species of large trees so far within the Arctic Circle. It is of far more importance than any or all the arguments I have used about the Glacial period. It admits of but one explanation. The long Arctic nights are caused by the obliquity of the ecliptic, and only by the lessening of that obliquity can they be shortened. There is no reason to believe that this vegetation could have been fitted to endure extreme cold. It belongs to many different genera, and the greater part are not those that are now characteristic of cold regions. For these, according to Heer, we should have to go to the very Pole itself. At the same time, in North Greenland, flourished a flora that could only live where frost was unknown. Many of the same species lived much further south along with subtropical forms that prove that the climate of Central Europe was then both much warmer and more equable than it now is, and Heer considers that the mean temperature of North Greenland would have to be raised at least 29° F. to enable the Early Tertiary flora to flourish there.

I have shown that a great increase in the obliquity of the ecliptic would produce the cold of the Glacial period, let us now consider what would be the effect of a great decrease in that obliquity. Would it tend to produce conditions favourable for the growth of vegetation up to the North Pole? It will simplify the question by investigating how far an entire obliteration of the obliquity would ameliorate the climate of the Arctic regions. The present position of the axis of Jupiter proves that there is nothing impossible in the supposition that that of the earth may also have been perpendicular to the plane of its orbit. The immediate effect would be the equalisation of night and day all over the world. With twelve hours' sunshine and twelve hours' darkness the seasons would disappear, or rather every parallel of latitude would have but one. At the equator alone would the sun rise directly overhead at noon. In the temperate zones would reign perpetual summer; within the Arctic circle perpetual spring. In Central Europe sub-tropical vegetation might then flourish. In North Greenland the sun every day would rise to a height of 20° above the horizon.

The forms of the continents were very much the same as they are now. It is probable that the Gulf Stream exercised the same sort of influence as it does at present on the climate of the North Atlantic, and it is significant of that influence that the most northern Early Tertiary forests have been found in Spitzbergen, whose shores it now laves. The flora of North Greenland suggests that a branch of the Gulf Stream also flowed up along its western coast. With twelve hours' sunshine, ice could not accumulate in Baffin's Bay, and it is not improbable that some of the warm surface currents of the ocean then found a passaget hrough Davis's Strait towards the Pole. Under such circumstances the west coast of Greenland might have its mean temperature raised as much as Prof. Heer thinks is necessary. During the day twelve hours' sunshine would give it the heat of a mild summer's day, and at night the warm currents flowing past its shores would prevent the occurrence of frost. The sequoia and the magnolia might then flourish and perfect their fruits in North Greenland; and even at Spitzbergen the Gulf Stream might cause frost to be unknown; but there the sun would rise to such a small altitude that the climate would not be warm enough excepting for hardy northern trees.

Whilst the cold of the Glacial period and the heat of the Early Tertiary period might thus be caused by a great increase or a great decrease respectively of the obliquity of the ecliptic, the extreme point to which the ice reached southwards, as in America, and that to which vegetation reached northwards, as in Spitzbergen, were both due to geographical conditions still in existence. In both periods we have evidence that the isothermal lines were deflected far northwards by the Gulf Stream, and that the east coast of America was much colder than the west coast of Europe. The evidence is overwhelming that the primary causes of these great oscillations of temperature were changes in the direction of the earth's axis; and, fortified by the conditions that we see obtain in the other planets, we may ask astronomers to reconsider the question of the possibility of these changes having taken place. Additional data respecting the exact figure of the earth have accumulated since the problem was last treated. The irregular figure of the earth must affect the result; and it is not probable that the effect of the attraction of the sun and the planets upon an irregular equatorial protuberance can cause a perfectly circular movement of the poles. None of the other movements of the heavenly bodies are circular, and why should this one be? The weak point in Lieut.-Col. Drayson's theory is the assumption that the imaginary line that the pole of the earth traces in the heavens is that of a circle. Through removing the centre of that circle from the point first fixed by other astronomers to another, he accounts for the cold of the Glacial period, but offers no explanation of the heat of the Early Tertiary period. He has, however, informed me that the curve really traced may be that of an ellipse or of a spiral, the time over which accurate observations have been made not being long enough to determine the exact figure. Geology teaches us that the obliquity of the ecliptic has been much greater and much less than it is now, but with the cause of these changes it cannot deal. This must be left to astronomy to decide, and I doubt not that the solution of the question will be attempted, and, notwithstanding its difficulty and intricacy, accomplished.

I have now come to the end of my argument. I have had more than one object in view. Besides trying to make plain what I considered the fundamental cause of the great oscillations of temperature, of which we have such abundant proofs in geology, I desired also to indicate the vast scope of the enquiry that the study of the Glacial period involved. It is not simply a question of scratched blocks and transported boulders. The whole physical geography of the world has been affected by it. Man's early history and his present distribution are intimately connected with it. Not only the valleys and the fiords of the north, but the great plains of Europe and Asia were produced by it. Even the existence of our continents may be due to a succession of Glacial periods that have from the earliest geological times disturbed the equilibrium of the earth's figure, and the volcanoes and the earthquake shocks of the present day may be occasioned by the slow recovery from the last disturbance of that equilibrium. Viewed in these lights, the history of the Glacial period has yet to be written; and whoever has time and ability to take up the study will find it one of extreme interest.

In treating the subject as I have done I know I must with many have weakened my argument by introducing questions not directly bearing on it. They will turn to some text-book and find it stated that the greater part of England was submerged 2000 feet below the sea in Glacial times; or that the secular cooling of the earth is an incontestable physical necessity; or that in some other way I have propounded a scientific heresy. They will fail to see that the main argument is not affected by these auxiliary theories, and they will decide against me. The human mind falls back on precedent and authority, and an original investigator must expect that every step he takes will be disputed. And in many ways the result is most beneficial, for the theories that survive do so because they have an innate vitality that carries them through all opposition; and those that cannot stand the test soon succumb to the chilling blasts of gusty criticism.

There are others, however, who will consider the argument strengthened and not weakened by these subsidiary speculations, for they know that it is characteristic of a true theory, like that of gravitation or the undulatory theory of light, that it explains numerous facts not originally contemplated when it was first suggested. Many must have been led to adopt the beautiful theory of the origin of species by natural selection, through finding that it afforded welcome help in the solution of problems in natural history besides those that Darwin first sought to explain by it. And I claim for this theory that it shows these signs of truthfulness. It not only explains the grand facts of glaciation and of the Arctic Tertiary flora, but it throws unexpected light on such far removed and seemingly unconnected facts as the traditions of a great deluge, the production of volcanic eruptions, and the growth of coral islands. It is the problem of human knowledge to bring the accumulating facts of the world's history through all time into one consistent and harmonious chain of consequences, and I trust I may in this paper have contributed towards that end.

II. LOSS OF LIFE AT SEA.

By Rear-Admiral FISHBOURNE.

THE loss of life and property on the high seas having grown to such enormous proportions that it became the duty of the Government to enquire whether much of this did not arise from preventable causes, we were not surprised therefore that a Royal Commission should have been appointed, or that they should have presented us with an instructive and highly valuable Report.*

Notwithstanding that, on the whole, the Commissioners have arrived at wise conclusions, their Report bears the marks of having been produced under pressure, the Commissioners working against time, and that some aspects of the subject were new to them. We are glad to give prominence to some of their recommendations, which we think cannot be too strongly insisted on:—

That "the Board of Trade should interfere only when there is ground for suspecting some gross mismanagement, and, whenever the case for detention may appear doubtful, to direct the attention of the ship-owner or manager to the circumstances which have attracted official notice."

"That the Marine Department of the Board of Trade should be revised and strengthened."

Some additional nautical assistance "is requisite for the due performance of the duties now entrusted to the Board. A legal adviser exclusively belonging to the Department is also essential."

"It will be the duty of the Board of Trade to check the negligent and to punish the culpable ship-owner, but it is desirable that these functions should be performed without harassing the great body of ship-owners, who, by their ability and indefatigable energy, have contributed to the prosperity of the Empire." And while they wisely deprecate any transference of responsibility from the ship-owner to the Executive Government, they add—"It is the duty of the ship-owner to keep his ship in a seaworthy condition, and to select competent officers and crew."

While the Commissioners insist upon the propriety of ship-owners being exempt from vexatious and injurious interference, they recommend that their natural and just responsibilities should be enforced, and even made more stringent: thus they say—They "think that in analogy to the principle involved in the eleventh section of the Merchant Shipping Act Amendment, 1871, the ship-owner's liability for damage to property or person should be unlimited in cases where the death of the seaman or the damage to person and property has been occasioned by the ship having been sent to sea in an unseaworthy condition, unless he proves that he—or those to whom he commits the management of his business—used all reasonable means to make and keep the vessel seaworthy. He should also, in these cases, be made liable, under Lord Campbell's Act, to the family of the deceased seamen." They "are also of opinion that any provision in a bill of lading or other agreement, having for its object or effect to avoid or limit the liability of the ship-owner in the cases just referred to, ought to have no legal validity." They "think that the ship-owner should *not* be enabled to recover his insurances, whether under a time or voyage policy, when it could be shown that he or his agent had not done everything reasonably within their power to make and maintain the ship in a seaworthy condition, and that unseaworthiness occasioned the loss."

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* Report of Royal Commission on Unseaworthy Ships.

Fixing such grave responsibility upon the ship-owner makes it imperative on the Government to protect him against that growing spirit of insubordination, carelessness, and ignorance on the part of the crews. With this view the Commissioners recommend enactments and prosecution under them of captains, mates, or men, whose defaults have occasioned the loss of life or property, and they properly recommend—for the owner cannot be well made responsible for acts committed abroad to which he is not privy—that the eleventh section of the Merchant Shipping Act, 1871, should be amended, and be made expressly to extend to the master of the vessel; for it is very important to avoid any doubt that the master who, without justifiable excuse, leaves port with his vessel in an unseaworthy condition, renders himself amenable to the criminal law.

The Commissioners justly attach great importance to instituting, in every case of disaster, a searching and impartial enquiry, to ascertain whether the casualty is owing to the faulty construction of the vessel, to bad stowage, to circumstances connected with the navigation, to the incompetency of officer, or to the neglect or misconduct of the crew.

No mere preventive or repressive measures can wholly meet the real difficulty which largely arises from a low moral standard amongst both seamen and officers, some of whom have worked their way up from the ranks, and have not had equal advantages with others; therefore it is with unmixed satisfaction that we view the approval given by the Commissioners to the training ships, and the duty of the Government to aid such; and we still further rejoice that the present First Lord of the Admiralty has given official recognition and approval to the view of the Governmental obligation.

In some respects these training ships may not be thought an equally good school for seamanship, but it is a far better nursery for the growth of bone and muscle, and far more for the formation of character, and for establishing moral and religious principles,—the only sufficient guarantee for good service and good citizenship,—while they receive an education which in every particular is germane to the duties they will have to perform when they are fairly embarked in the duties of their profession.

Once more, we rejoice that England is not content to have the residuum of other nations to man her ships, and still less content to play the stepmother with her own offspring, but undertakes to fit them by the tens of thousands to fill fittingly noble duties opening the door to higher destinies.

We do not know whether the Commissioners allude, in their opening remarks, to the agitation originated—perhaps not wisely, yet not without cause—by Mr. Plimsoll, when they say—"Public attention has been eagerly directed to some of the above precautions." Other sources of danger have been altogether unnoticed. "Adding a summary of official enquiries into wrecks and casualties, excluding collisions, shows that from the year 1856 to 1872 inclusive, a period of seventeen years, while 60 ships were known to have been lost from defects in the vessel or in the stowage, 711 ships were lost from neglect or bad navigation."

In this the Commissioners are scarcely fair, for they admit that these statistics are hardly reliable, and on page 4 they prove them to be utterly worthless, for there they prove that during the first nine months of the operation of the Merchant Shipping Act, 1873 (36, 37 Vict., c. 85), out of 286 ships surveyed, 256 were found to be unseaworthy,—234 from defects in the ship or equipments, and only 22 from being overladen."

This return shows how comparatively futile would have been the special provisions intended by Mr. Plimsoll's Act for determining the load-line and limiting the deck loads to summer seasons, while his recommendation for general inspection would have been intolerable; and if this army of Inspectors were not more capable than some of those now employed, the results would have been most injurious to the trade and dangerous to the sailor. If Mr. Plimsoll would benefit the sailor and his country he must seek other support than Mr. E. J. Reed's exaggerated statements about vessels being loaded till the water washed over their decks. Mr. Reed joins Mr. Plimsoll in his attempts to prevent deck loads being carried, because of raising the centre of gravity of ships, which is the occasion of their capsizing, and this top weight is further added to in the winter months by the seas that then come over their bulwarks. It requires, however, but a small acquaintance with every-day facts on the Thames to have known that barges and yachts are to be seen with the gunwhales immersed and inclining almost with the water up to their masts, and without a shadow of danger. So, also, the Dutch galliots, on the high seas, allow the water to have free access to many parts of their decks amidships, and this because they have wisely supplied a sufficiency of the cardinal element of safety, viz., stability.

The Commissioners come to the conclusion that "the reserve of buoyancy," which they define to be "the proportion which the capacity of the water-tight and solidly-constructed part of the ship which is above water bears to the capacity of the part immersed," should "mainly guide in the settlement of a load-line." Further on they add—"Any rule of freeboard founded on surplus buoyancy gives to a vessel of light scantling an advantage over a stronger vessel. Thus the inferior ship would by law be allowed to carry the heavier cargo. Such an enactment would not contribute to the safety of life at sea."

"There is no general agreement as to a rate by which the requisite amount of reserve buoyancy could be determined, and it appears that, except under definite circumstances, it is not a determinable problem."

"The proper load-line in each particular case depends not only upon the principal dimensions of the ship, but also upon her form and structural strength, the nature of her cargo, the voyage, and the season of the year." Again, they say—"Discretion as to the proper loading of his ship must be left to the ship-owner, or, under his directions, to the manager, on whom the responsibility rests for sending the ship to sea in a seaworthy condition, which responsibility it is inexpedient to diminish. They have therefore come to the conclusion, not without regret, that we cannot prescribe any universal rule for the safe loading of all merchant ships."

This is all true so far as it goes, but it is very unsatisfactory that it goes no further. They omit from the list of considerations that should influence the load-line, the ruling one—that of the disposition of the cargo: this wrongly stowed renders nugatory, as regards safety, all care that may have been bestowed upon other points. Then there is a looseness in the use of the term *reserve buoyancy* that ought not to occur; the mere fact that a part of the vessel, as she floats, is above water, by no means proves that this is reserve buoyancy—it may be absolutely without buoyancy, and may be, so to speak, only waiting for an opportunity to take its natural place below. The ship may have become, by bad stowage, what Sir Edward Watkin styled—when speaking of vessels designed by Mr. Reed—a "ship upside down."

Buoyancy, to be efficacious in that degree, must be real. Buoyancy is the hydrostatic pressure that supports any body floating in a fluid. It is equal to the difference between the mean specific gravity of the floating body and the fluid in which it floats multiplied by the displacing volume.

It is clear, from the above exact definition, that unless that portion of the body which is temporarily above water is of less specific gravity than the water in which the body floats, it does not possess buoyancy, and cannot give any support.

We strongly object to the season of the year being taken as a consideration when fixing the line to which a ship should be loaded, as it implies that it is proper that a ship should be less safe in summer than in winter, forgetting that she may cross the Tropics, and thus pass from summer into winter, and that she may, and they do, frequently meet with abnormal seas and winds in summer; besides, ships stowed on such erroneous principles as these cannot be otherwise than proportionably unmanageable and ineffective, and do not bring profit to the owners.

The Commissioners seem to recognise the faultiness of their definition of buoyancy, or rather the erroneous ideas that are current about it amongst those who inculcate that it matters not where the buoyancy is placed, so only it be sufficient in the aggregate; for they say (p. 6):—"Double bottoms for water-ballast are attended with danger, because when the cargo is taken on board these spaces are emptied of water, and this may tend to capsize the ship. There are many cases where the double bottom affords additional security; any legislation on this matter would therefore be inexpedient."

We fear in too many cases the possible advantage of a double bottom in very remote cases, such as taking the ground, is made the justification of establishing a permanent danger of capsizing, as alluded to by the Commissioners, but further of effecting the danger it provides against, for the existence of an inner bottom leads to scamping the work on the outer, and reducing its strength far below that which it ought to be.

This tendency to capsize was designed to be the normal condition, as we learn, of many of our vessels of war, and evidence has been given concerning some of them, that their outer bottoms were too weak to bear their own weight when grounded, and so they would not have taken a first-class, if any, certificate at Lloyd's.

We are glad to find the Commissioners recognising the danger of light bottoms, and of ill-adjusted deck-loads, and therefore inferentially condemning that system advocated by Messrs. Froude, Reed, and others, of giving ship's little stability, not simply in the abstract, but actually contending that little stability was a prime element in *safety*, than which no system can be less well founded or more dangerous; but we think the Commissioners ought to have gone further, and, instead of saying that they "could not give any universal rule for the safe loading of merchant vessels," should have stated that the cardinal element in a ship's safety and effectiveness is in great stability, and for this their centres of gravity, when loaded, should never be much above the centre of their immersed body. This would dispose of the difficulties of low freeboard and water-line and deck-loads.

The Canadian authorities except deck-loads of deal on the principle, reasonable enough, that they are light as compared with the other part of the cargo. Deck-loads of cotton and of deals even are dangerous when the general cargo is only cotton and light wood, for in such vessels it is nearly impossible with such to get the centre of gravity low enough for safety. Nor is there any danger of uneasiness in recommending so low a centre of gravity in merchant ships, since their proportion of breadth—which is the disturbing and antagonistic element, so to speak, as respects motion—is small, and the tendency is to making it smaller, for the sake of greater cargo-carrying and small masts and small crews.

The Commissioners have properly recommended that responsibility shall be shared out to all concerned in due proportion—captain, mates, crew, and owner,—but, while insisting that the safety of a ship cannot be secured without care and vigilance be given from her first design to her unloading at the port of destination, they fail to mete out to the architects and builders, who uniformly claim the lion's share of success, any share of responsibility or blame for the failure of designs: and yet most of the great catastrophes lie at their doors, and many of the smaller are due to their misleading, or to their, and that of other peoples', assuming that in dealing with purely nautical subjects they can dispense with nautical experience and knowledge, and can decide all nautical questions without the assistance of seamen. We rejoice that the Commissioners are no parties to the discrediting of nautical knowledge in naval questions.

As Mr. Froude's theories concerning the best mode of preventing the rolling of ships in a seaway have been extensively applied in our navy, it has become a matter of great importance to demonstrate their erroneous character, and to point out the great danger of continuing to apply them.

Let us now examine the principles on which Mr. Froude proceeds.

He alleges that "the effort of stability is the lever by which a wave forces a ship into motion; if a ship were destitute of this stability," he says, "no wave that the ocean produces would serve to put her in motion."

He offers the following explanation and illustration in proof of the correctness of the above view:—

"I regret you did not notice the conclusive experimental proof I exhibited of the fundamental proposition, that the surface of a fluid when dynamically inclined is virtually level to a body which floats on it, and as it really lies at the root of the whole theory of the behaviour of a ship amongst waves, I am anxious that not only my re-statement of it, but reference to the experimental proof of it, should appear also."

Mr. Froude then gives the following as the experimental proof of his fundamental proposition.

"If a shallow dish of water be suspended by three equal strings brought to one point, so that it is level when it hangs at rest, then if the meeting of the strings be taken in the fingers, the dish may be swung about in any direction whatever without spilling the water; indeed, it is impossible to spill the water so long as the suspending lines are kept tight by the operation; and if a stable floating body be placed in the water it will carry its mast at right angles to the water, whether it owes its stability to deeply-stored ballast, or to a broad plane of flotation."

Now there are other conditions that must be complied with before the floating body can be swung in "any direction whatever;" and it is not impossible to spill the water "so long, merely, as the suspending lines are kept tight by the operator," for the water will inevitably be spilled if the motion be not communicated concurrently, slowly, and equably to the containing vessel and the water: moreover, when the model ship is placed on the mimic sea, it also must be so placed before motion is communicated to the dish and water; for if all are not put in motion at the same time then the motion of the ship and water will not synchronise, for they will be subject to different amounts of force, and "the fundamental proposition" will be seen not to hold good.

The principles contained in the experiment are the same as those which obtain in the milkmaid's trick of swinging her pail of milk over head, and in the practice of the sailor in swinging his sounding lead over his head: in each case sufficient velocity in rotation must be given to overcome the gravitating force, or the milk will be spilled, or the sailor will be advised of his stupidity by a good thump. In the case before us, unhappily, it is the sailor who is punished, because another, the philosopher, ignores the existence of a well-known law.

Mr. Froude, having stated his fundamental proposition that "the surface of a fluid when dynamically inclined is virtually level to a body which floats on it, overlooks the essential difference between the conditions which obtain in his illustration and those which obtain in a ship in a seaway."

Because the floating body on his mimic sea is subject to the like dynamic forces with the water, and moves with it, he assumes that a ship in a seaway "is subjected to the same dynamic force as the wave on which she floats, and might be treated as a surface particle on it," which is simply impossible, as a ship is subject to other forces than those which the sea give out; and though she is subject to the motions of the wave, she does not accept the wave motion, for if she did, her motions would always synchronise with those of the wave, which they never can entirely do.

And notwithstanding that the floating body on his mimic sea would continue to move with the water (if once set in motion with the water, and is not subject to any other force, as a ship amongst waves is), whether the form of body were V- or U-shaped, whether the centre of gravity were high or low, whether the body had one keel or a dozen, small or large; and this, because there is no motion of the particles of water amongst themselves, nor of the floating body amongst them, other than those produced by the one power; they *all* being moved by the same power at the same time, and to the due extent, according to their relative positions in the orbit of motion, the upper parts or particles moving in the least arcs, though their motions may be modified by the law of gravitation; yet every one of the above changes in form, in weight, and in disposition of the weights, would affect the motions of a ship in a seaway in facilitating or in retarding motion, so that a ship's motions could not synchronise with those of the wave. Moreover, as the motions of the water in his illustration are different from those of water in wave motion, in which the upper parts or particles moving with greater velocity than the lower, there is no analogy to the conditions of the water in the dish; also as the parts of a ship are substantially rigid, and one part occupying space amongst the fast-moving particles, and another part amongst the slow-moving particles, it is impossible that the ship could move wholly with either one set or the other, or with both more than in part, and in some varying degree. Again, while in his illustration the containing vessel, the sea, the floating body, and *all* the water are moving together under the impulse of the centrifugal force; in the case of a ship at sea there is a portion of every ship that is down amongst the unagitated water, *below* wave motion; this portion of the body would resist and retard motion, and prevent a ship from accepting and moving with the waves, if otherwise free or disposed to retain her masts perpendicular, to the slope of the wave. Indeed, were it true we should not have any such cases as ships and boats being rolled over by waves, which are only too numerous, and will be more so in proportion as his ideas on that subject are adopted.

In fact in no case are the circumstances of a ship amongst waves similar to those of the illustrative model, nor can a ship ever "accept the aggregate dynamic conditions of the sea on which she floats," nor still less "be treated" with any regard to truth "as a surface particle of the wave on which she floats."

There are also further limitations on the motions of a ship in a seaway, to which the model in Mr. Froude's illustration is *not* subject.

Thus the amount of a ship's motions are the result of the wind on the sails, or on the hull and masts, or the inertia of the masts and sides of the hull, together with the ever-varying resistances from onward, and from rotatory motion; while in the case of the model there is but one motor, and one influence ruling, that and gravity being common to both.

Nor can it be conceived with any approach to correctness that the place of the operator's hand, as specified in Mr. Froude's illustration, "holding ever so tightly the strings," can represent the *point* of buoyant suspension of a ship, or even on the principle that her motions are analogous, as they are not, to those of a conical or other pendulum, nor that the three strings represent the lines of buoyant power meeting in the hand of the operator.

These forces, as the ship rolls or is inclined, are continually changing in direction and in amount, as it were, first on one string then on the other; therefore the point of suspension and length of strings are continually changing.

Lastly and conclusively, the conditions of Mr. Froude's illustration require that the ship shall be suspended by three or more strings or something analogous; and not that only, but that the wave or waves on which she partially and temporarily floats shall also be like as in the tray experiment suspended; not that alone, but that each wave as it arrives up to and passes under her shall be likewise so suspended; nor that only, but that the ship, the sea, even to the depth to which any portion of the ship is immersed, and a portion of which is always below the limit of the depth of wave motion, and all altogether shall be supported on three or more strings, and be all moved together and to the same extent by an invisible hand communicating motion from one point *above* the ship; whereas everyone must know, if he knows anything about the motions of ships at sea produced by waves alone, that instead of this imaginary hand or power moving the ship from *above*, the ship is always in a seaway moved from *below* without the intervention of strings, and that the *water* and not strings or a hand or anything analogous is the motor, and which, unlike the hand and strings in the experiment, *never* succeeds in *wholly* imparting its own motion to the vessel floating on it.

There is nothing of that which is required by Mr. Froude's theory, which is therefore a mere fiction; and though there doubtless is a superstructure of superior mathematics built up by him, yet as it is *baseless*, his conclusions could not be otherwise than erroneous and proportionably deceptive, and the adoption of them could only lead to the damage of our ships and danger to the lives, if not also to the death, of some of our seamen; and so it has proved in the sad fate of the loss of the *Captain*, and in the great danger to which other ships were exposed, as the writings of the advocates of these erroneous views have practically admitted.

Then he says, "The effect of stability is the lever by which a wave forces a ship into motion. If a ship were destitute of this stability no wave that the ocean produces would serve to *put* her in motion, *whether* the stability be due to deeply-stored *ballast* or to the broad plane of flotation."

Yet the effort of stability is originated by, and is *not* the originator of, motion; it is the effort put forth in resisting motion, and increases in amount with each increase of the extent of the motion up to a certain point.

Stability may be obtained either by deeply-stored ballast, or by a broad plane of flotation, or in part by both; but they are unlike, and in some degree opposite in their operation and effects; yet here also Mr. Froude treats them as though they were strictly alike in their operation and in their effects.

No doubt the plane of flotation in proportion as it is wide when the sea is motionless, therefore not a motor, gives a very high degree of stability; but when the water is moved into waves, that same broad plane becomes proportionally *an instrument* but not a motor, by which the waves give much greater motion to the ship, *they* being the power that puts the ship in motion—making her unsteady yet not unstable, as is argued by Mr. Froude, for in that case she would capsize, as his model also would if unstable, and would not preserve its masts perpendicular to the surface of the water, this new hypothesis notwithstanding.

Unquestionably if a ship be without stability, as Mr. Froude suggests, she will be unstable and not steady, but will do the opposite to that stated by that gentleman, viz., "not be rolled by any sea whatever," for she will certainly roll over, as was near being demonstrated in the case of the *Vanguard* and her sister ships; no doubt she will not roll in the sense of rolling from side to side, for being once rolled, and not possessing stability to bring her back to the perpendicular even when the rolling force ceases to act, she will roll over.

Still less is there any tendency in ballast or a low centre of gravity "to *put* a ship in motion;" its action is to preserve the motionless condition of the ship, and it possesses no leverage till by motion it has been pushed out of the perpendicular by some force which is neither that of stability nor of ballast, ballast itself never being even like the broad plane of flotation, an instrument in originating greater motion; on the contrary, when it is pushed out of the perpendicular, its effort is always to bring the ship back to the motionless condition, and its effort in this direction is greater in proportion to the extent of the disturbance from the perpendicular, a disturbance originated by that other force; and in proportion as the ballast or centre of gravity is lower does it tend to limit the extent of the motion, which is the opposite of the action of a broad plane of flotation.

In proportion as the stability is derived from the weights being placed low, and relatively great as compared with the *smallness* of the plane of flotation, a ship will roll less and less.

Many have seen this fully illustrated in the steadiness or freedom from rolling with which a boathook floats in the midst of waves.

This condition is also well illustrated by a floating target, which remains vertical because of its studiously low-placed centre of gravity and small plane of flotation.

Let anyone take a boathook and load it still further near the hook so as to push it well down into the water, and arrange that the stave or wood handle shall be as small as will consist with floating the added weight if placed in the midst of high waves, it will be found to float, and continue to do so nearly upright, the waves as they pass running up the stave without inclining it further or rolling it, and why?

Because the great weight acting low down with all the force of the stability which it gives out, in consequence even of the small incline from the perpendicular, is exercised, not as Mr. Froude suggests, "to put the ship or handle in motion," but to prevent further motion except in the vertical plane; and the ballast succeeds in preventing motion, because the whole plane of flotation, or more properly the volume subject to immersion and emersion as the waves and hollows between the waves pass, and which alone constitutes the instrument through which the waves act to *put* the ship in motion, is little or nothing compared to the force of the low weight, and to the action of the body low down in the undisturbed water, *resisting* motion. The obvious course therefore to be pursued, when it is desired to reduce the amount and the rapidity of motion in ships, would be to keep the plane of flotation comparatively small, the centre of gravity low, and make the depth half the breadth or as great as would consist with general good qualities, and the object for which any particular ship might be destined.

Instead of which two other courses are recommended by Mr. Froude and his school, viz., to raise the centre of gravity as compared with that in ships generally, and to distribute the weight outwards and on to the sides.

We may examine the effect of such recommendations with advantage.

The condition of ships in a seaway is misunderstood, as is the effect of increasing the inertia of their sides; the condition is not as supposed by Mr. Froude, that of a ship moving with and as a particle in the wave; when waves are large or are steep, and are travelling fast across a ship's path, she has no time and may have very little tendency to accommodate her seat in the water to the surface or level of the wave, so that it becomes oblique to the ship's waterline, as in Fig. 1, producing an inequality in the pressures, which are the cause of the rolling motions. All the conditions of the pressures on the inclined body in still water, shown in pages 73—4 of "Our Ironclads and Merchant Ships," holds good equally as to the upright body with reference to an inclined surface of the wave.

FIG. 1.

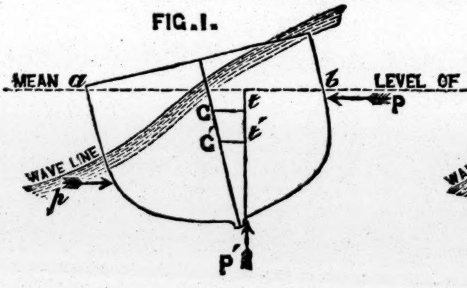
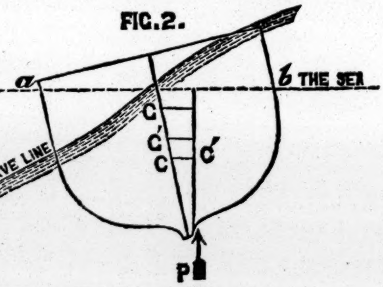


FIG. 2.



In comparing two ships, or the same under different conditions, we must assume the wave to be alike in all particulars, then consider the variables and their consequences.

The disturbing wave force then is the same, whatever may be the form or distribution of weight in the ship; yet the effect of that force will vary considerably as the above are changed.

We will suppose Fig. 1 to represent the cross section of a ship, which, by weighting her sides, is prevented from inclining with the wave, or from accommodating herself as much as she otherwise would to the surface of the wave, which is represented by the wave line curve.

There is a complete change in the amount of the pressures from those that existed when the sea was level and the ship upright; suppose the new pressures to be represented by the arrows at P P′, and *p*.

The water has accumulated at P, raising the point and increasing the amount of lateral pressure on that side, while the water has partially left the other side, decreasing the pressure there and lowering the point to *p*. These two pressures act as a couple to rotate the ship; that at P high up, and more high as the wave is higher and its upper particles moving with greater velocity. That at *p* low and more low as the hollow of the wave is deeper, and their power to rotate the ship increases in the same extent.

Then P′, moved over to the side of greater pressure, acts up on a line normal to the earth and perpendicular to the mean level of the sea through *t*; G in the first instance representing the position of the centre of gravity, and G *t* the perpendicular distance from the line of pressure; this also tends to turn the ship in the same direction as the other pressures.

It is clear that the greater is the accumulation of water the further P′ will be carried over, and the greater will be the lever G *t*, which in this case tends to overset; consequently, any arrangements that would tend to increase this accumulation of water would be injurious, and might be dangerous.

Now suppose the centre of gravity lowered to G'_1, then the upsetting lever will be G′ *t*′, which is obviously shorter than G *t*; consequently, lowering the centre of gravity reduces the power of the wave to incline or roll the ship, and tends to limit the arcs rolled through.

Or suppose, though it is less accurate, these three forces to be represented by one at P passing up through the centre of figure C′. In Fig. 2, it is clear that the greater is the accumulation, the greater distance C′ will be moved out from a line passing through the middle of section or ship; therefore the greater will be the distances of a perpendicular to the earth passing through C′ from the centres of gravity at G or at G′: obviously, as in the former case, in proportion as the centre of gravity G is lowered, so the disturbing lever is decreased.

It is clear also that the greater is the breadth, the greater distance will C′ be carried out, and the greater will be the disturbing lever.

The greater is the accumulation of water also, the further is C′ carried out, and the greater is the disturbing lever. The greater is the effect of Mr. Froude's recommendations, the greater becomes the upsetting lever.

The higher also the centre of gravity is, the longer is the disturbing lever, and the greater is the extent of the motions and danger of upsetting. (See p. 490 for a more definite proof.)

The more rapidly the wave is moving, the greater is the difference between the motions of the upper and lower particles; therefore the higher up is the lateral thrust, and the danger greater from a high centre of gravity.

It may be said that, in lowering the centre of gravity, the point round which the ship turns will be changed, and therefore c′ will not occupy the same place under the altered conditions. This is true, but the fact is in favour of our argument, for, in proportion as the centre of gravity is low, the radius will be shorter, c′ therefore will be moved out a less distance, and the injurious influence of the wave motion will be proportionably less.

To assume, therefore, that the metacentric height is a measure of the disturbing force is erroneous.

It is indispensable for a ship's safety, as it also is for her complete efficiency, that she should possess a considerable metacentric height; then to reduce this by raising the centre of gravity, under an impression that the arcs of roll will be reduced, is erroneous and an unmixed evil.

It is not true, as stated by Mr. Froude, that "the effort of stability is the lever by which a wave forces a ship into motion. If a ship were destitute of this stability, no wave that the ocean produces would serve to put her in motion, whether the stability be due *to deeply stored ballast* or to a broad plane of flotation."

If, however, the metacentric height is made unduly high by great proportionate breadth, or by placing a cargo of metal on the ship's floor, then the motions will be too *rapid*, though short, in which case the evil may be got rid of, with benefit and without danger, by raising the centre of gravity. But still there is a minimum of metacentric height, below which it cannot be reduced with safety.

Ships must yield to the waves, and all arrangements and designs should be to facilitate their doing so slowly and equably. The effect of Mr. Froude's plan is to put off the inclination till the wave accumulates in such force that it will sweep the decks, force the ship over suddenly, and possibly overturn her, if she be not previously "swamped."

Therefore the metacentric height, or the distance of the centre of gravity from the metacentre, must not be taken as giving the measure of the disturbing force, for in lowering the centre of gravity the metacentric height is increased, but we have seen that in proportion as the centre of gravity is lowered so the disturbing action and extent of motion is limited; and therefore the proposition to raise the centre of gravity, with a view to reduce a ship's motions, is erroneous, and in proportion as it is raised it is dangerous.

No doubt the other proposition, that of increasing the moments of inertia by distributing the weights laterally, if it gives the wave, so to speak, more to do to lift this weight, on the one side, it must be remembered that there is, besides, an accumulation of water on that side which overcomes these moments; but there is a withdrawal of water support from the other side, and that side tends to fall in search of support, and, the more the weights are extended out on that side, the greater is the force from that cause to rotate the ship.

Obviously, their lateral distribution of weight increases and renders a ship's motions in a sea-way dangerous in proportion as the waves are larger and more steep, the cure for which is concentration of weight laterally and distribution of weight downwards.

This has been immemorially the wise practice in bad weather, to send small yards and masts on deck, to fill empty tanks, and get the guns well in from the sides, and notoriously the ships rolled less and were easier, and yet the stability was increased by two causes, viz., lowering the centre of gravity, and increasing the surface stability by reducing the weight of the sides, increasing their buoyant power. Consequently, the only distribution of weight that is safe in bad weather is that which is downwards, and therefore raising weights off the bottom, and concentrating them vertically, is wrong on their own principle, as that reduces the inertia, and doubly wrong on the principle we contend for, viz., the propriety of lowering the centre of gravity and distributing the weights vertically.

The proposition to get rid of the difficulties arising out of the adoption of a wrong principle by increasing the breadth and endeavouring to limit the motions this will entail, by deep keels, will be a failure—

First, it will not effect the object intended. We have passed the limit of practicable depth for useful ships, and any increase of breadth will increase the disparity between depth and half breadth, and occasion, as the ship rolls over and rolls back, a greater rise and fall of the centre of gravity, this action will occasion, so to speak, rolls, independent of those produced directly by the wave by which the total arc of roll will be increased, together with an increase of rapidity of roll, which keels cannot prevent if they will mitigate.

This rise and fall it was that caused the frightful rolling of Sir William Symond's ships.

What will happen in a ship with weight concentrated on her sides, high centre, and low freeboard, say on the L'Aghulas's bank, with a weather current and steep waves, will be that the great inertia of her sides will resist their rising to the wave, but finally it will lift her suddenly, but a large body of water will break on board, and will rush over to the other side. She will then have three forces tending to turn her, the beam sea lifting the upper side, the weight of water weighing down the lower side, and the current running to windward below, making an adverse couple to roll her over, all which will be facilitated by the empty cells in her bottom. If she is not rolled over, it will be that a good Providence has intervened; if she does capsize, we shall know who to hang.

The same danger will arise from a weather tide or current, and from great tidal and solitary waves.

There are two other elements that materially affect the rolling motion, viz., length and depth: the greater these are the greater is the resistance to rotatory motion. Then, in proportion to the height of the waves, the depth of agitated water increases, and so does the portion of the ship down in unagitated water decrease, and with it the limitation to great motion decreases.

It is universally admitted that the lower the centre of gravity is, when once the ship is inclined by a force, the more rapidly will this low centre bring the ship back to the perpendicular, but then, also, in contradiction to Mr. Froude's view, must its power, of necessity, be greater to resist the force of the waves, acting through the solids of immersion and emersion to rotate the ship: also, the wider the plane of flotation, the greater will be the leverage and power of the sea to put a ship into greater motion; and, *cæteris paribus*, the higher the centre of gravity, the less will be the leverage and power of the ballast, or low centre of gravity, to resist motion. Therefore it is that in proportion as the centre of gravity is high, and the vessel broad, the effect of the sea in rolling the ship both rapidly and through large arcs is greater! But obviously it is not desirable that a ship should roll through large arcs, for in that case the armour to be a real protection must cover the whole side thus exposed from time to time at each roll, while, in addition, to a greater degree, would the unarmoured deck be presented to the enemy, and the employment of guns on such a deck would be made proportionably difficult; still less is it desirable that these large arcs should be performed rapidly. Therefore the plane of flotation should not be greater than is necessary to give the requisite stability without having the centre of gravity too low.

The correctness of the above reasoning was abundantly shown in the behaviour of the ships forming the Channel Squadron in 1871, the large ironclads exhibiting to great disadvantage when compared with the small wooden and unarmoured ships, though it is proverbial that, *cæteris paribus*, the larger the ship, the better the weather she ought to make.

Some of the ironclads rolled as much as 62°, some 56°, others 50°, while the much smaller *Topaze* rolled but 22°.

Again, while the little *Topaze* rolled but 22°, the large and most approved ironclad *Minotaur* rolled 39°, and the large and fine *Northumberland* rolled 38° under similar circumstances.

True, Mr. Reed attributes this difference to the existence of that which he thus admits, though he had previously denied it in former examples, the fact that the *Topaze* had larger masts and yards for her size than the others.

But we may ask if large masts produce such remarkably favourable and, we may add, essential qualities, why not increase the size of the masts of the *Minotaur*, as they would serve other useful purposes in addition? and why have shortened the masts in the *Vanguard* class?

The fact is that the effect of the difference of the size of the masts in mitigating the rolling motions of the *Topaze* was little compared to the disturbing effect of her broad plane of flotation, and but for which she would be easier in fine weather also, as she is so much easier in rough weather than the ironclads. The proportion of breadth to length in the *Topaze* was 1 to 4·7, while in the *Minotaur* it was 1 to 6·7; the proportion of breadth to length of the *Lord Clyde*, which was the vessel that rolled through the largest arcs, was also 1 to 4·7. In fact, the metacentric height of the *Topaze* was greater than that of most of the ships present on that occasion.

The rolling of these ironclads is without parallel amongst wooden vessels, and is only approached by those of the very worst form, and yet the ironclads are, in some respects, of a better form than the wooden ships; the excessive rolling, taking them as a whole, being mainly, though not entirely, due to the concentration of weight on the sides and want of stability, the fact being that no ironclad possesses near the amount of stability assigned by the ordinary mode of calculating. This arises from the excessive overloading of the solids of immersion and emersion, by the armour and by the other concentration of weight toward the sides, the consequence of which is that these portions of the ship are deprived of buoyancy, and they afford the ship little or no support; so when she is inclined they tend little or nothing, compared to their volume, to move the centre of buoyancy over to one side or the other, in rolling,—so for want of this support, amongst others, the ship rolls through large arcs, once the sea has overcome the inertia of the sides.

And this evil would increase in each, as they were lightened by the consumption of their coals, provisions, and water, and their stability thus reduced. Great as were the arcs of roll recorded of these ships, the probability is that, with the exception of the *Lord Clyde* or another, very little of their weights were out; so they were in the best condition!—how bad the best!

The loss of the steamship *Tacna* is an instructive comment on the foregoing arguments, and a complete condemnation of the latest of the unsafe theories, *i.e., that "a vessel whose stability is greatest when placed bottom upwards may yet be perfectly safe when floating upright.*"

The description of the loss, founded on a "mature consideration," as given by the Court, is "the loss of the ship *Tacna* was due to an excessive loading of her main and hurricane decks, which, with the combination of circumstances adduced in evidence as having arisen at the time of altering course for the port of Los Vilos, caused her to heel over until, falling on her beam-ends, she filled and foundered."

The Court condemned the Captain for "not having made known in a more especial manner to his employers the *crankness* of his vessel, and for not having exercised sufficient care in the amount of deck cargo."

Here is a ship "floating upright" till she comes to turn, when the centrifugal force, which is always greater as the centre of gravity is higher, capsized her. But will any reasonable man say she was safe?

Even a Pacific sea or a squall would have done equally what the centrifugal force did.

We have no question here as to a deficiency in the reserve of stability which a high side is said to give. If we ask, Where was her *reserve* of stability that she so went over? echo answers, Where?

The captain was justly condemned for loading the ship as he did, knowing her to be crank; but he was not responsible for the latter, and he might in some degree be excused when persons who are said to be the greatest authorities on such subjects say that crankness is a benefit and not a defect, so they have recommended little stability!

The ship was lost, like the *Captain*, because "she was not endowed with sufficient initial stability;" for this the naval architect was primarily to blame, and merited condemnation by the Court as having contributed to the loss of life which occurred. Had the ship been lost within Chilian jurisdiction, it appears the captain also would have lost his life.

The ship never was seaworthy from deficiency in her initial stability, and whether that proceeded from direct design, from a double bottom, and from not making due allowance for that, or from an error in calculation, the naval architect alone was to blame and ought to be held responsible; and it will be useless legislating for the safety of life and property if naval architects are allowed to do as seems good in their own eyes without being held accountable. To say that the deck load capsized this vessel is no exoneration, as the Froude-Reed system provides that ships in a seaway shall have a deck load of the greatest, most generally damaging and dangerous kind, that of water, which will rush to the lower side and guarantee an upset; for in providing so far as they could that a ship shall remain upright in a sea, they provide that the waves shall roll into and over her.

These gentlemen may say, Oh, no, we think there ought to be sufficient stability to enable ships to carry the amount of sail with which they are furnished. Let us consider this:—

The maximum pressure of wind must be assumed, acting at the given leverage occasioned by height and area of sail; in fact, the stability must be equal to the force shown by the wind couple in the case; and more, as a force coming suddenly is said to occasion double the inclination, the stability should be double this: then the stability should be sufficient to bear the ship up against inevitable deck loads of water, or a blow of a heavy sea striking her when she is inclining, such as proved fatal to the *Captain*, because of her small initial stability.

The metacentric height, while that is the accepted mode of estimating stability, ought not to be less than 6 feet for smaller, and 5 feet for larger sailing vessels, somewhat less for steam vessels with little more than fore and aft sail.

It is clear no such quantity was contemplated for the *Invincible* or *Sultan* and a fleet of other men of war, which a deck load of water from a sea or other weight would have cut short in their career while hardly begun if they had not been heavily ballasted; and yet a vessel of war shorn of the power to carry deck loads would often be useless; thus on an 800-ton steamer it depended to keep up communications with the army in Kaffraria and the colony, to throw in reinforcements, provisions, horses, and munitions of war, carrying sometimes as many as 800 levies with their stores and provisions, and this all on deck. Yet this vessel was proverbially an easy vessel. Numberless instances of the necessity for such a property could be given.

It may be affirmed also with confidence that various exigencies occur when such a power is necessary in merchant ships, such as taking crews off sinking vessels, and yet no such power was provided for in the many ships that have capsized because of their having deck loads.

It may be said that we object *in toto* to double bottoms, though they are necessary, because the thin iron skin is more easily perforated than is the wood planking of wood ships.

We do not object to any proper use of a second or inner bottom, to make iron ships stronger and safer against injuries from taking the ground or from torpedoes, or to facilitate repairs: such ships require it, as their bottoms are far less strong and less calculated to bear injury than those of wood ships; and we doubt not those iron vessels, with deep iron bilge-keels, will be specially liable to damage; nay, more, that these very keels on taking the ground in many cases will serve to tear a large portion of the lower bottom out, if they do not injure the inner or upper bottom through the frames, thanks to the little-stability system smuggled into the navy to "*check* rolling."

We do, however, object to raising these inner bottoms unduly for the purpose of raising the centre of gravity from an idea that it is wise to do so.

We object to making the bottoms of vessels light, on the grounds of cheeseparing parsimony, and then assuming that this makes no difference in the properties of the vessel, that the bottoms are equally heavy, though empty, with all other parts of the immersed body, even when they are crammed full, and then assuming that the results of calculations made on such an hypothesis can be otherwise than erroneous, deceptive, and proportionably dangerous. That they are so we have proved in pages 34 to 44 of "Our Ironclads and Merchant Vessels,"* and need not repeat it here.

We have also demonstrated that in proportion as the spaces between the bottoms are large and empty it will be necessary for safety to lower the centre of gravity; it will be necessary, also, with the same object, to lower the centre of gravity in proportion as the sides are more and more loaded and deprived of buoyant power.

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* Our Ironclads and Merchant Vessels, by the Author. Harrison and Sons.

The oscillations of wide vessels with heavily weighted sides are liable to carry them beyond the upsetting angle; for this reason, also, their centres of gravity would require to be kept lower.

It will be asked, What! was there nothing in past experience to justify the adoption of the Froude-Reed system? No facts, no experiments proving its correctness and safety? absolutely none.

The one complaint in which all were for long agreed was, that our ships were deficient in stability, from which they always suffered in comparison with French and Spanish ships. This led to the introduction of Sir William Symond's system, and the success of his ships was entirely due to their greater stability, which they maintained till greater stability was given by his opponents.

His system was exploded because his stability was, for the most part, obtained by a broad plane of flotation; therefore his ships were more at the mercy of the waves, and their motions were extravagantly extensive; the more so that they were without a low centre of gravity to limit the range of motion.

When steam was introduced the engines and boilers took the place of ballast in keeping the centre of gravity low, more necessary because of the reduced breadth to obtain speed economically, and necessary because of the great reduction of stability when the latter part of the coals were being consumed.

A lower centre of gravity is necessitated in merchant vessels by the fact that many of them are obliged to carry deck loads.

As great stability and much of it by a low centre of gravity was adopted as indispensable by common consent alike of sailors and naval architects, the result of long extensive and conclusive experiments, and represented by a metacentric height of 5 to 6 feet or more, an architect giving much less to a design would have been justly esteemed as foolish or criminally ignorant. The laws of nature being unchanged, how can it be otherwise now in giving metacentric heights, such as 2·6 feet, 2·3 feet, 2·2 feet, and even 1·5 feet under other much more unfavourable conditions? There was nothing in the facts offered in justification of the course pursued.

As we now propose to show:—Mr. Reed wrote, "the steadiness at sea of our ironclads is due to their want of stiffness or stability;" also that it had been found that raising the centre of gravity tended to "check rolling, and that it was a popular fallacy to suppose the armour-made ships roll."

It was recommended by Mr. Froude with a view to reduce the tendency to roll to raise the centre of gravity of ships, and to distribute their weights towards their sides.

Sir Spencer Robinson accepted the principle involved in these statements and writes:—"It is remarkable, that according to theory, the rolling of the ships being very much influenced by the position of the centre of gravity with regard to the metacentre, and by the moment of stability, the order of rolling of the French ships as observed follows that law."

The heat of the metacentre above the centre of gravity in these ships is as follows:—

Armoured	*Solferino,*	4·5 ft.	Armoured	*Couronne,*	5·37 ft.
Unarmoured	*Napoleon,*	4·9 ,,	,,	*Invincible,*	6·36 ,,
Armoured	*Magenta,*	5·0 ,,	,,	*Normandie,*	6·59 ,,
Unarmoured	*Tourville,*	5·31 ft.			

"This," he says, is "precisely the order of merit they have taken as to rolling." The *Solferino* rolling least and the *Normandie* the most.

Now we unhesitatingly affirm that there is nothing to justify the conclusion that these gentlemen have come to, viz., that the greater rolling was caused by the greater metacentric height, but the reverse.

We accept the statement as to the performances of these ships as given by Sir S. Robinson.

1st. He says "that the *Solferino* is superior to all the ships of both squadrons," but this includes the *Achilles,* yet her metacentric height is only 3·1 feet, while that of the *Solferino* is 4·5 feet; therefore the theory breaks down; nor is this all, for the *Solferino* is nearly one-third smaller, *i.e.*, of 2600 tons less displacement, than *Achilles,* and, therefore, might reasonably be expected to roll more instead of less; and while they have the same extreme breadth, which is the disturbing dimension, the *Achilles* is 90 feet longer!

The decided superiority of *Solferino* must be due to the lower centre of gravity and greater metacentric height, and the weights being on her bottom tending to limit the arcs rolled through or to "check rolling."

Again he states "that *Achilles* is quite as good as the *Napoleon;*" what! not much better? Why the metacentric height of *Napoleon* is 4·9 feet, that of the *Achilles* only 3·1 feet! The disturbing force of *Napoleon* may be represented by the cube of 55, that of the *Achilles* by the cube of 58; but then the latter is 147 feet longer and has only 4400 tons greater displacement! Moreover, the French ship not being an ironclad did not possess the alleged soporific of weighted sides. The theory here is doubly in fault. The true explanation is a low centre of gravity, great metacentric height, and weights on the floor.

Then the *Achilles* is said to be rather superior to the *Magenta*, when she ought to be very superior, being so much larger, that is, 90 feet longer and 2600 tons more displacement, with same breadth of beam, *i.e.*, an equal disturbing element. Here, also, the theory fails, and the explanation is a lower centre of gravity and weights on the floor!

Also, *Achilles* is stated to be decidedly superior to the rest of the French squadron, excepting the *Tourville*. Why, the metacentric height of this ship is 5·3 feet, that of *Achilles* 3·1 feet; she is without the soporific of armour, is 140 feet shorter and 4000 tons less displacement than *Achilles*. Again the theory is doubly wrong.

Achilles is superior to the rest of the French squadron, *i.e.*, to the *Couronne*, the *Invincible*, and the *Normandie*. Really we are surprised that a direct comparison should have been made; she is 110 feet longer than *Couronne*, and 3300 tons more displacement, 118 feet longer than each of the other two, and 3600 tons greater displacement than one, and 3890 tons more than the other. That these did not roll very much more than her is due to their higher centre of gravity and greater metacentric height.

For, comparing the French ships amongst themselves in respect of their metacentric height, there is not even the show of reason; for, first, the two unarmoured ships must be struck out, as it is now admitted that they roll less than ironclads. The theory as to the contrary is now abandoned by Mr. Reed—reason and experience have re-obtained their sway.

The French ironclads take their places in the order of their size, the largest rolling least. The two first are sister ships, as also the two last, but of a lower class; the proportion of *breadth* to length in these last is greater than that of the other three, therefore they ought to roll more. The *Magenta* is 1100 greater displacement than the *Normandie*. Then the *Edgar** is said to be "inferior to the *Napoleon*,

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* We take the metacentric height of *Edgar* to be the same as her sister ship, 4·6 feet.

Tourville, *Solferino*, *Achilles*, and *Magenta*." The inferiority cannot arise from her metacentric height, for it is equal to that of the *Napoleon*, but less than that of all the other French ships. Why should she be inferior to the *Napoleon* and *Tourville?*" Because being 3 feet wider, she is subject to greater disturbance. The other ships are all so much larger, especially the *Achilles*, that no comparison can be made; the *Achilles* is 140 feet longer, and 4000 greater displacement.

We take the metacentric height of *Black Prince* to be the same as that of her sister ship *Achilles*, that of *Hector* is 4·6, and *Black Prince* is a sister ship to *Achilles*, and ought not, as far as we are informed, to be different from her.

The *Defence* is said to be better than the *Hector* and *Prince Consort*. With the equal length the *Defence* has 2 feet less breadth than the *Hector*, and with 7 feet greater length she has 4 feet less breadth than the *Prince Consort*, therefore we should expect the latter to have greater motion than either of the others.

Then the *Black Prince*, with her smaller metacentric height, 3·1 feet, is admitted to be inferior to the *Solferino* and *Magenta* with their considerable height of metacentre, one 4·5 the other 5 feet.

We have before examined the facts offered in support of the theory as drawn from the comparison of the rolling of a number of English ironclads, and have shown they range themselves as respects their rolling exactly in the order of the proportion of breadth to length, the vessel with least proportionate breadth rolling least. The *Minotaur*, the largest and best ship, with a metacentric height of 3·8 feet, superior to the *Bellerophon* with only 3·2 feet of metacentric height. The *Pallas*, a ship of Mr. E. J. Reed's, the worst roller, her smaller metacentric height not checking the rolling so much as the great metacentric height of the *Prince Consort*, 6·1 feet.

It was natural for *Pallas* to roll; she had such a great proportionate breadth of plane of flotation.

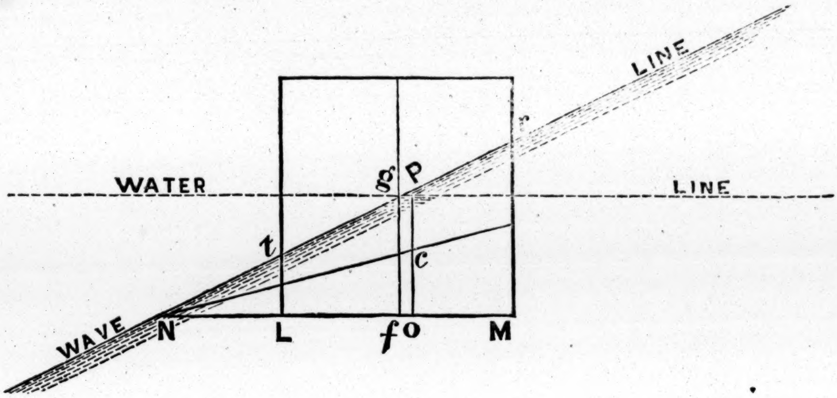
Raising weight did not cure, but the contrary; it made her rolling worse.

Obviously the theory that induced these gentlemen to damage a fleet of ships by reducing their stabilities has no foundation in experience, in reason, or in science:

And as this system might have been effectually tested in the course of an afternoon, not one of these unsafe ships should have been built.

Proof of Proposition stated page 478.

If, instead of supposing the floating body to be inclined at any given angle in smooth water, we suppose the surface of the wave to be inclined to the mean surface at an equal angle, it will be evident that the reasoning with reference to the pressures, pages 73 and 74, will hold good.



Take—

$$Mr=b,\ Lt=a,\ NM=c,\ NL=d,\ NO=X.$$

Take the usual formula for the centre of gravity, and integrating between the limits c and d—

$$X=\frac{\int xy\delta x}{\int y\delta x}$$

$$y=\frac{b}{c}x$$

$$\int xy\delta x=\frac{b}{c}\int x^2\delta x=\frac{b}{c}\frac{x^3}{3}$$

$$\int y\delta x=\frac{b}{c}\frac{x^2}{2}$$

$$\frac{\int_d^c xy\delta x}{\int_d^c y\delta x}=\frac{h}{3}\frac{c^3-d^3}{c^2-d^2}=$$

$$\frac{2}{3}\frac{c^2+cd+d^2}{c+d}$$

$c+d=24 \qquad 3(c+d)=72$

c^2+cd+d^2

$324+108+36=468$

$2(c^2+cd+d^2)=936$

$$X=\frac{936}{72}=13.$$

Formula for the equilibrated lever $\frac{b^3}{12S}-a.$

Centre of gravity of the system being above the centre of displacement.

In this formula, b = the line of intersection of the cross section of the floating body when upright with the surface of the water; S, the figure of displacement when the body is upright; a, the distance between the centres of gravity and displacement when the body is upright.

In this case that of a block 12 feet square as described, page 72, &c.—

$$b=12 \text{ and } S=6\times 12.$$

$$\frac{b^3}{12^2\times 6}=0{\cdot}5$$

$$op=\frac{13}{2}=6{\cdot}5.$$

$$oc=\frac{op}{2}=3{\cdot}25.$$

g being the centre of gravity of the floating block fg, evidently = 6.

$$a=b-325=275$$

$$\frac{b^3}{12S}-a=0{\cdot}5-2{\cdot}75=-2{\cdot}25.$$

The stability being negative in this case; but as the centre of gravity is lowered, a is diminished till it becomes equal to $\frac{b^3}{12S}$, when the stability is that of indifference; when a is less than $\frac{b}{12S}$, the stability becomes positive and the righting lever increases, while the upsetting lever remains constant; the conditions in other respects remaining unaltered.

III. THE LUNAR ATMOSPHERE AND ITS INFLUENCE ON LUNAR QUESTIONS.

By Edmund Neison, F.R.A.S., &c.

PERHAPS no question in connection with the condition of the moon's surface has excited so much attention as the existence, or not, of a definite lunar atmosphere; and it must be conceded that it possesses the highest importance, when considered in its relation to the many problems of great interest to be decided in connection with the present and past history of our satellite.

Curiously enough, although the most diligent and expert selenographers, with hardly, perhaps, but one exception, have long recognised both direct and indirect evidences of the probable existence of an atmosphere, yet among astronomers in general it has been usually considered as definitely established that the moon is to all intents a perfectly airless globe. For even if it has been granted that it may possess, perhaps, the merest trifling trace of gaseous envelope, it has been considered established, for a certainty, that this must be far less dense than the most perfect vacuum that has been artificially produced by an air-pump.

There does not appear to exist, however, sufficient grounds for this widely entertained belief in the non-existence of a lunar atmosphere having been proved; for apart from the necessity, not observed in this case, of cautiously accepting positive conclusions based entirely on purely negative results, the subject has never been examined in a sufficiently complete and accurate manner. In consequence, many of the criteria that have been regarded as demonstrating the non-existence of a proper lunar atmosphere should never have been taken into consideration as applicable. Most of these negative results, which have been relied on as showing the absence of any gaseous envelope to our satellite, are only applicable to an atmosphere similar in density to our own. But such are the favourable conditions prevailing upon the terrestrial surface that if an atmosphere whose mass and that of the planets were in a constant ratio were to exist on every planet, the earth's would be far the densest of any, excepting the giant of the family Jupiter. But on the moon the most unfavourable conditions prevail for the density of atmosphere; and under the conditions supposed, and these conditions the most probable imaginable, then the lunar atmosphere would possess at the surface a density only about one-fiftieth of the density at the terrestrial surface. Notwithstanding this, however, as before stated, the ratio between the masses of the atmosphere and planet would be the same as on the earth. All criteria directed, therefore, against an atmosphere similar in density to our own, are quite inapplicable to the moon's; and an examination of any of the works treating on the question will show how many are of this class.

Further examination will also show that we have probably to deal with even a less density than some one-fiftieth of the earth's. The moon's mean density being only three-fifths of the earth's, the moon's surface is, in proportion to the lunar mass, over six times greater than on the earth. Accordingly the atmosphere would be subjected to the action of a surface more than six times as great as has been on the earth. There can be little question but that at one time the terrestrial surface must have exerted a powerful action on our primitive atmosphere, and have absorbed and locked up much of what constituted our earliest atmosphere, for on no other conditions can be explained the known nature of the terrestrial surface. But as the moon's primitive atmosphere must have been exposed to the action of more than six times the extent of surface, it would appear not improbable that her atmosphere must have lost much more in proportion than the earth. Even, therefore, if both planets originally possessed atmospheres whose masses, and those of the planets, were in similar ratio, the moon's, thus exposed to a far greater absorbing surface, must in the end have lost the most; and so the residue left possess a smaller ratio to the mass of the planet than on the earth. We can easily see, therefore, how—by the action of this proportionately six times greater surface—it is possible for the moon's atmosphere to have been reduced to one-sixth the ratio to the mass of the planet that we find on our terrestrial surface. It is true that this would be a very difficult question to in any way prove, or even to maintain in its integrity, but this is immaterial for our present purpose; and it may well be admissible that, even had the planets started with the same proportional atmosphere, that of the moon may have been reduced to one-sixth. As the lunar atmosphere, under equal conditions, has been shown to be only one-fiftieth of the terrestrial density, reduced to one-sixth it would possess only one three-hundredth, or possess a density expressible on the earth as one-tenth of an inch, or some $2\frac{1}{2}$ millimetres. Now such an atmosphere is possible.

But such an atmosphere is not small; the only thing small about it is its density, as compared with the very dense atmosphere of the earth, while its real magnitude and mass is immense; and the mass above equal portions of the surface is not so much smaller than on the earth.

To ascertain what atmosphere is possible on the moon, it is necessary that we should express, by means of a few simple equations, the conditions any atmosphere would have. As, however, the derivation of these equations from the known physical laws, with respect to gases, would involve the application of more complex mathematics, it will be dispensed with. Throughout, it may be observed, it is assumed that the moon's atmosphere would be essentially similar in composition to the earth's, with perhaps a much larger percentage of carbonic anhydride, which is to be expected.

Let a = the radius of the moon in miles;

x = any height in miles above the surface;

δ_0, p_0, t_0 = the density, pressure, and temperature of the atmosphere at the lunar surface, and δ, p, t the same at height x.

And put for the constants involved—

$\boldsymbol{a}$ = 0·000294, multiplied by the density of the lunar atmosphere in terms of the standard terrestrial density of air;

ε = 0·003665, the expansion of gases for 1° C.;

l_0 = the height in miles of a column of the lunar atmosphere of density δ_0, at temperature t_0 that would—under the action of gravity at the moon's surface—exert a pressure of p_0. From this—

$$l_0 = 32{\cdot}6837\,(1+\varepsilon\, t_0) \text{ miles.}$$

Finally, for brevity, write θ for $\dfrac{\boldsymbol{a}}{l_0}$.

Then the equations expressing the physical condition of the lunar atmosphere may be written—

$$x = l_0\left\{(1-f)\,u+2f\,(1-e^{-u})\right\} \quad . \quad . \quad . \quad (1)$$

Where e is the base of the natural logarithms—

$$\delta = \delta_0\, e^{-u} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$p = p_0\, e^{-u}\,(1-f+f e^{-u}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\frac{1+\varepsilon\, t}{1+\varepsilon\, t_0} = 1-f\,(1-e^{-u}) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

And lastly, for the horizontal refraction r_0, expressed in seconds of arc, undergone by a ray of light in passing through the atmosphere—

$$r_0 = \frac{a(1+a)}{\sin 1''}\sqrt{\frac{\theta\pi}{2}}\left\{1-(f-a\theta)(\sqrt{2}-1)+f(2-\sqrt{2})\right\} \quad . \quad (5)$$

where f is the function on which the rate of decrease of temperature depends.

Were the temperature of an atmosphere constant throughout, the density and pressure would, in accordance with Mariotte's law, diminish together in geometrical proportion with increase of altitude. The temperature is, however, not constant, but varies inversely as the altitude; so it becomes necessary to assume some law for this decrease, and this will evidently depend primarily upon the supposed temperature of space. It has been generally assumed, by those who have lately written on this lunar subject, that the temperature of space is near the absolute zero of heat,—in contradistinction to Fourier and Hopkins, who regarded it as but little below zero, which is preferable. Whatever value may be adopted, the corresponding value of the function f is given by the equation—

$$f = 1 - \frac{1+\varepsilon t}{1+\varepsilon t_0}, \quad . \quad . \quad . \quad (6)$$

as the temperature of space would not be reached until a considerable distance from the moon.

These equations, it is seen, involve the temperature of the lunar surface, and from Lord Rosse's determinations it appears that the moon's surface may reach 200° C. For the minimum value the temperature reaches during the long lunar night very different values have been ascribed, and it is evident that much may depend on the temperature of space assumed. It has, moreover, been generally thought that any lunar atmosphere would, from its extremely small density, exert little influence in retarding the radiation of heat from the moon; but it has been overlooked that the thickness of the envelope of atmosphere, as well as its density, must be considered. Now the thickness of the lunar atmosphere is for this purpose many times greater than the earth's, and this counterbalances its tenuity; accordingly the retardation of radiation would not be much less than on the earth. As during our long terrestrial polar nights, from six to ten times the length of the lunar night, the mean temperature is not usually many degrees below zero, it will therefore be probable that the temperature of the moon's face will not fall either much below zero, for there is more heat to be lost than on the earth. It is, moreover, to be remembered, that the proportion of carbonic anhydride in the moon's atmosphere would be probably much greater than in the earth's, and this powerfully retards the radiation of heat. From these considerations, then, we have at sunrise the moon's surface at any position near the equator, the temperature about —20°, which will increase until a little after full moon, when it will have risen to about 200°, and will then slowly decrease, until at sunset it will be about 60°. During the lunar night the temperature will fall slowly until a few days before sunrise, when it may become as low as —30°, rising again to about —20° at sunrise. But it is evident that as the temperature is solely due to the solar rays, these must be the extreme results; and that for any place away from the lunar equator the climate will be much milder, never rising to so high a temperature, as the sun will not be in its zenith, nor ever falling so low.* This circumstance has usually been overlooked.

The temperature of the lunar surface having been therefore approximately determined, the equations can be at once applied to find the conditions of the greatest lunar atmosphere possible under known circumstances.

An examination of all the methods that have been applied to detect a lunar atmosphere shows that all the most delicate —and, in fact, only applicable ones, to such an atmosphere as we have seen would probably exist—are based upon the refraction of light by the atmosphere. And the condition under which this refraction is most marked is that which requires attention, namely, the horizontal refraction. And even of these selected methods, the most delicate and only thoroughly reliable one is that based on the retardation of the occultation of a star, at the dark limb of the moon, by the refraction caused by the light traversing the atmosphere. For when the diameter of the moon is known, the exact

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* The values in the text are arbitrary, but have been arrived at from theoretical consideration in conjunction with Lord Rosse' determination, and can be expressed by the following equation, which gives the theoretical results for the maximum and minimum taken. Assume ξ is the solar altitude at any spot, and increases from 0 to its maximum, and then falls to 0, at which it remains while the sun is beneath the horizon. Put ζ equal to 300 degrees at sunrise, and rising uniformly by 2π during each lunation; then if λ be the latitude of the spot we have roughly—

$$t_0 = 170^\circ (1 - A \cos \xi + B \sin \zeta - C \sin \lambda).$$

It has been taken that $A = 0{\cdot}9$. B, which varies as the cosine of the latitude, is 0·28 at the equator, and $C = 0{\cdot}08$, which values but represent the theoretical probabilities.

time of the disappearance of the star, were there no atmosphere, is easily calculated; and if, therefore, it is found that the observed time of occultation is later than the calculated, then a lunar atmosphere—of a density that would cause this retardation—appears certain.

Unfortunately the moon's diameter is not known with exact accuracy for certain, although it is so within very small limits; and this somewhat complicates matters. Sir George Airy has determined, from transit circle observations, what is regarded as a very accurate value for the moon's telescopic semi-diameter. This value has been considered very slightly too great, owing to the effect of irradiation at the lunar limb caused by the contrast between the bright moon and the sky; although a careful examination of the observation of the lunar diameter shows that this is questionable, and renders it doubtful whether the variation found with different instruments is not due solely to the spurious telescopic disc.

Observations made with the Great Equatorial, at Greenwich, during the solar eclipses of July, 1860, and December, 1870, enable this to a great extent to be rectified, by showing the minimum value for the lunar semi-diameter to be some 15′ 34·00″—a value in agreement with the observations of Greenwich and Oxford. For it is apparent that during a solar eclipse there is an absolute reversal of the effects of irradiation, as they must then—to a much greater extent—diminish the apparent diameter of the moon than under ordinary conditions they can ever increase it,—as the contrast between the dark moon and the extremely bright disc of the sun is vastly greater than the contrast between the comparatively feeble light of the moon and the sky. Moreover, as the point to be determined in observation during a solar eclipse is where darkness ceases, we have absolutely the fullest action of irradiation, together with the effect of a maximum spurious telescopic disc; while under ordinary conditions of observing the moon's limb with the transit circle a smaller spurious disc comes into play, and it is questionable whether the effect of irradiation on these observations has not been over-estimated. In, therefore, observations of a solar eclipse the moon's apparent diameter is reduced to a much greater extent than under ordinary conditions it can ever be increased; and we have, accordingly, the value for the moon's minimum diameter before stated.

The minimum lunar semi-diameter may be considered as 15′ 34·00″, although it is possible that the true diameter is somewhat larger, as indeed is indicated by the observation made during the eclipse of 1870, the most favourable one of the two. It remains now to see whether, with this lunar semi-diameter, the observed occultation of stars by the moon's dark limb indicate the presence of any lunar atmosphere; for the occultations at the bright limb are very inferior, and the re-appearances at both limbs are almost valueless.

From a large number of observations of occultations of stars by the moon—made between 1850 and 1873 when very satisfactory lunar places had been obtainable, and observed at Greenwich, Oxford, and Cambridge—it has been shown* by the author that the lunar semi-diameter derived from the occultations at the dark limb of the moon is less than the adopted minimum semi-diameter by over two seconds of arc, corresponding usually to from five to ten seconds of time. The retardation of an occultation being less than twice the horizontal refraction,† this would indicate the existence of an atmosphere exerting a horizontal refraction of a little over one second of arc; and the possibility of its existence appears beyond question, for no other method of detecting an atmosphere approaches this in delicacy.‡

The equations on which the physical conditions of the moon's atmosphere depend may at once be employed to find the density of a lunar atmosphere whose horizontal refraction will be a little greater than one second at the temperature of about zero. By putting the density of the moon's atmosphere at about one three-hundredth of the earth's, it will be found to satisfy this condition, keeping the value of f moderate. It will also be found that, as the average temperature of the moon's bright limb is high, the refraction exerted by the atmosphere is only about one-half of that at the dark limb; and, if allowance is made for the effects of the spurious telescopic disc at whose edge the star will disappear usually, it will be found that the observations of occultations at the bright limb give agreeing results.

The great difference in the density of atmosphere arrived at for somewhat similar retardation of occultation—for Sir George Airy has long seen the possibility of an atmosphere being the cause of this—arises from a very simple cause. From merely looking at the question in a general manner it has been always considered that for a similar surface density the refraction on the moon and earth would be alike; but a mere inspection of the complete differential equation to the refraction of light through an atmosphere shows that the refraction for equal surface densities varies approximately for small refraction as the square root of the radius of the planet and the action of gravity on the surface, and on the moon both, are much less than on the earth. On the moon accordingly, for equal surface densities, the refraction is only about one-fifth of what it would be on the terrestrial surface, and, this circumstance having been overlooked, the density of the atmosphere has been greatly underestimated.

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* Monthly Notices of the Royal Astronomical Society, vol. xxxiv., p. 356.

† In the Monthly Notices, vol. xxxiv, p. 20, the late Sir John Herschel's value has been taken, but it has since been found that the above is correct. This requires the suppression of the first sentence of the bottom paragraph of that page.

‡ That further discoveries and observations may possibly modify this conclusion is of course unquestionable, but this constitutes no reason for altering any deduction to be drawn from the circumstances as they exist at present.

It would be useless to travel through the numerous methods that have been referred to as showing the non-existence of any lunar atmosphere, not of much greater tenuity than the most perfect vacuum of an air-pump, for they are not applicable to such an atmosphere, although it could hardly be called even a vacuum, much less a perfect air-pump vacuum, but a few general remarks will suffice. The effect of the atmosphere on the phenomena presented by the moon would be imperceptible in all except the single case of the retardation of occultations, for the amount of refraction is insufficient to in any way distort or alter the appearance of a star or planet seen through it, and, similarly, any effect it might exert upon the spectroscopic observations of our satellite would be marked by the greater action of the terrestrial atmosphere. The sole action it would exert during a solar eclipse would obviously be to simply augment the solar diameter to a very slight extent, but, as this latter is not known with accuracy, this would be undetectable; neither would any new lines be revealed by the spectroscope in observations like Mr. Stone's,* for there would be no new substance to give them, as would be indeed very improbable. Finally, it may be observed that the rays, after refraction through the atmosphere, would not be convergent like after passing through a lens, for, the refraction diminishing with the height, rays from different altitudes would be divergent. This atmosphere, which at first sight might appear so rare as to be unworthy of notice, is not so in reality, for it is of much greater proportionate dimensions than the earth's, which it even exceeds in actual volume, counterbalancing its less density. Consequently, the amount of atmosphere acting on the moon's surface is not much smaller than on the earth, and, as far as a single square mile of surface is concerned must be estimated by millions of tons. Moreover, from these considerations as to the horizontal refraction exerted by a lunar atmosphere, it is evident that, were its density a little less than one-thousandth of the terrestrial density, the retardation at the limb during occultations would no longer be detectable from its being smaller than the probable errors of the method, and yet it cannot be questioned but that this atmosphere would still exert a most marked influence on the physical condition of the lunar surface.

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* Monthly Notices of the Royal Astronomical Society, June, 1874.

There are many selenographical problems dependent on the question of the existence or not of a lunar atmosphere, and prominent amongst them stands the absorbing subject, so often debated, of lunar changes. Though the configuration of the surface of the earth is undergoing a never-ceasing change from the action not only of volcanic forces, but also of the various atmospheric and aqueous causes, were it to be regarded from the moon, it is doubtful whether during the last century, with all our telescopic aids, any unmistakable alteration in the formations constituting it, or in its appearance even, apart from changes of tint, could be detected. Similarly on the moon there may be even more energetic action in constant work, without its making such vast changes in the appearance of our satellite as to direct our attention unmistakably to the circumstance of lunar, volcanic, and other energies being still in active progression. For such is our imperfect knowledge of the details of the configuration of the surface of the moon, that, were at intervals of a few years new volcanic cones, craterlets, and hills to come into existence, it is improbable in the extreme that they would be recognised as new formations instead of new objects detected.

Suppose the new manifestation of lunar volcanic energy took the form of some of those volcanic cones, as occur on both the earth and moon—a steepish hill, some hundred yards in diameter and hundred feet high, with a deep mouth of no great dimensions; or else, perhaps, gave rise to a new ridge of mountains, such as are so common on the moon, some miles long and a mile broad, covered, perhaps, with small crater cones not exceeding some hundred yards in dimensions—there is not one ten-thousandth of the moon's surface where these new formations would possess the slightest probability of being taken for the result of the action of present lunar volcanic energy. Or were, from some vast convulsion, a magnificent lunar crater to be formed, and cover the surface around with streams of lava, with a mouth two thousand yards in diameter and a thousand feet deep, its discovery would be improbable—nay, unless it occurred close to some few small localities, it is almost certain that it would not be detected as a new formation. Assuming, however, that one of these new formations were to appear, or some craterlet a thousand feet deep and several miles in diameter collapse into ruins, and granting one or two of the very few astronomers who study the moon's surface had for years sufficiently studied the small portion of surface where it occurred as to be sure it was a new formation, it would then be found impossible to prove it to other astronomers.

The actual effects of libration on the moon, in ever presenting the objects on its surface in new lights and at different angles, makes the lunar formations always variable in appearance to some extent ; while the general opinion of the powers of libration to change the appearance, visibility, and form of lunar objects, even near the lunar centre, is such that the most marked changes of appearance are ascribed to its effects. It would therefore require a most striking and wonderfully marked difference in the appearance of a large lunar formation to occur, before the existence of lunar volcanic, or other inherent energies would be admitted as demonstrated, or perhaps probable. There is, in fact, a certain amount of basis for this reluctance to admit the existence of lunar changes, for certainly the difficulty in the study of the details of the moon's surface is such as to render great caution necessary ; but it must not be overlooked, as is commonly done, that this in itself prevents the absence of any proved case of change in the lunar formations being accepted as a complete demonstration of the cessation of the action of all volcanic and similar forces on the surface of the moon.

Those astronomers who have made the details of the surface of the moon their study, are well acquainted with the almost absolute want of knowledge of the real minute details of the surface, and accordingly have long been convinced, with perhaps the exception of Beer and Mädler, that we are without any evidence of the constancy of the configuration of the moon's surface, with one exception, and this bar to the possibility of the moon's volcanic and other sub-lunarian energies being still actively at work was the supposed absence of an atmosphere. Volcanic action and no atmosphere seems inconceivable, as De la Rue has long remarked, but, grant the existence of the lunar atmosphere, and at present there is no evidence that the moon's volcanic energies are not tenfold more active than on the earth, and the importance of the determination of the probability of a lunar atmosphere is evident.

There is another question which is completely controlled by that of a lunar atmosphere. Most selenographers, and even the most cautious, Beer and Mädler, have recognised on the moon indications of the existence of some form of vegetable life, but, again, vegetation and no atmosphere is inconceivable; waiving the question of moisture as carrying us into too large a field, and granting the existence of an atmosphere, and most interesting problems are opened. For the researches of M. Paul Bèrt have shown that vegetable life is possible under atmospheric pressures only one-twentieth of the normal terrestrial one, and not so much more than is possible on the moon; considering, therefore, the wonderful adaptability to circumstances of vegetable life, who can say it is impossible at a pressure less than this? The moon's temperature need be no bar, for, although a fatal objection near the equator, it is no longer so nearer the poles.

Similarly with the many more recondite problems that the study of the moon's surface has given rise to—the streaks and rays of the full moon, the variability of the markings, floor, and craterlets of Plato, the white cloud-like coverings of Linne and γ Posidonius, the difference in the polarisation of the light reflected from the plains and mountains observed by Secchi, in their photographic power found by De la Rue, in their colour by Birt, in their visibility by Schmidt—all involve in their consideration the question as to the existence of a lunar atmosphere. It must be at once admitted that the existence of a lunar atmosphere facilitating the probable explanation of these phenomena proves nothing; for the circumstance, that granted a basis and all can be explained, is no demonstration of the truth of that basis. But neither must it be assumed, as is so often done, that the possibility of accounting for phenomena on other grounds shows the proposed explanation to be probably wrong; the nature of the two must always be considered. For example, it is possible to explain the photometric results of Zöllner as to the brightness of the moon by assuming either that the lunar surface is covered by a multitude of very minute steep conical hills, each of a definite slope, and arranged in a complex manner round two common foci, or else that it is covered with extremely small regular furrows of uniform angle, running from pole to pole at proper distances; and these hypotheses explain most satisfactorily all Zöllner's observations; but they cannot be considered as disturbing the explanation that the cause of the phenomena in question is simply the known irregularities on the surface, which also accounts for the observations, although not so well. It is, however, too generally considered that the possibility of two explanations of one circumstance weakens the probability of both.

Competent dealing with the problem of the physical condition of the surface of the moon requires a special knowledge of the minuter details of the lunar surface, rarely met with in combination with a proper acquaintance with the more theoretical elements involved in the subject, and, in consequence, questions have often been considered as standing in a different position to their real nature. A mere general familiarity with the principal configuration of the moon's surface and the phenomena to be seen is not sufficient in the hands of the most able theorist for the proper consideration of the complex problems involved, and it is not strange if the resulting conclusions are often not all that could be desired.

The study of the details of the moon's surface, until very recently long left in the hands of a very few indefatigable astronomers, from the interest inseparable from it is now attracting the more general attention it demands, and gives promise of before long placing us in a more satisfactory position with regard to the nature of the lunar details. The admirable work of Beer and Mädler, so far from being exhaustive as was long thought, brought us to only the threshold: Schmidt's forty years' work, gigantic as are the results, only reaches the minutiæ, and will require years of revision; while the real character of the objects now known to exist merely, is nearly untouched ground, and it is there where some of the greatest difficulties will be felt, for to evolve a theory as to their source is far different from detecting their true origin. But there is promise ere many years are over that we may be in possession of a sufficiency of exact knowledge of the features presented by the moon to enable some more satisfactory conclusions to be drawn as to the events of the past history, present condition, and immediate future of our satellite.

IV. BERYLS AND EMERALDS.

By Professor A. H. CHURCH, M.A., &c.

ALTHOUGH to the mineralogist the stones known as aquamarine, beryl, and emerald, constitute but one species, there are decided differences of optical properties, as well as minute differences of chemical composition, between them. Under the name "beryl" minerals of considerable variety of aspect are thus included; some specimens being nearly opaque and brownish-yellow, others being perfectly transparent and colourless, while others, again, are beautifully tinted with a pale sea-green hue. But other colours also occur, such as yellowish-green, pale blue, and even lilac. Still it is in the emerald, once thought to be a distinct mineral species, that the richest and most highly appreciated colour is found. The pure green of this gem has provoked several enquiries as to its nature, and, quite recently, Mr. Greville Williams* has taken up the enquiry, and published the first portion of his results. In presenting an account of this research to our readers, we will also give some facts derived from other sources concerning the transparent beryl and the emerald.

This mineral species occurs only in crystals, while such divergences from constancy of composition as it exhibits affect scarcely more than 2 parts out of 100 in any specimen. It is essentially a silicate of alumina and glucina, but it always contains a little iron, while chromium seems a constant ingredient of the true emerald, though occurring as a trace merely, not amounting to a half per cent when calculated as sesquioxide. Other metals have been recognised in some varieties of the beryl by different analysts, and the list will include calcium, magnesium, manganese, and tantalum.

The specific gravity of transparent pale beryls is, as nearly as may be, 2·7, so that it is decidedly denser than rock crystal. This fact may be beautifully illustrated by means of a solution of mercuric iodide in potassium iodide.† Such a liquid may be readily obtained with a sp. gr. = 3·0, but a solution not denser than 2·67 or 2·68, nor lighter than 2·665, is required in the particular case in point. In such a liquid a piece of flawless rock crystal floats, for its specific gravity is but 2·652 at the most, while all beryls, when free from any kind of imperfections, such as cavities, fissures, &c., appear to range in density from 2·69 to 2·705, and consequently sink. The same is true of the emerald when perfect, but the general presence of internal flaws in this stone, even though they may be very minute, lowers its specific gravity decidedly, and may even reduce it to that of rock crystal. It should be stated here that the specific gravity of beryl is not altered by any temperature short of that necessary to effect the fusion of the stone, and thereby to change its crystalline structure to the condition of a glass.

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* Proc. Roy. Soc., No. 145, 1873.

† Recommended by Mr. E. Sonstadt for such experiments, in the "Chemical News," vol. xxix., p. 127.

A word concerning the hardness of beryl will suffice. Pure, transparent, and colourless or pale specimens are harder than quartz, varying from 7·5 to nearly 8; yet the best emeralds—though they scratch quartz, and of course all kinds of glass and paste as well—are slightly softer than the paler varieties of the species, such as the aquamarine.

We now come to the question of the colour of the emerald, a question which has been lately studied with particular care by Mr. Greville Williams. The green hue of this gem is of a different quality from that of the precious beryl and the aquamarine,—indeed it closely approaches, in really good specimens, to the pure green of the solar spectrum. The colour is, also, far more permanent than that of some beryls, especially those of a yellow or greenish-yellow hue, which become bluish, or even pale blue, when heated before an ordinary gas blowpipe jet, long before they show any signs of incipient fusion. The emerald, on the other hand, when heated alone before the oxyhydrogen blowpipe, bears a bright red heat without loss of colour, and even when it begins to fuse the edges only become colourless and opaque, the centre remaining green. Even when fused, emeralds are kept at the maximum temperature attainable by means of the oxyhydrogen blowpipe, they retain an opalescent dull green hue for some time, and it is only after prolonged heating that they become at last transparent, and almost free from colour. Greville Williams has also found that the addition of chromium sesquioxide to a fused emerald bead which has become colourless produces only a dull green colour, or, at the best, a colour decidedly inferior to that of the emerald. Cobalt oxide gives a fine blue tint when similarly added to an artificial mixture having the composition of the natural beryl or emerald, and this blue colour is not altered by the high temperature of the oxyhydrogen flame. With such artificial beryl composition other coloured glasses may be made, one of the most interesting of these being that containing didymium oxide, which imparts a lively pink colour to the fused bead. Moreover, this beryl glass shows the black absorption spectrum of didymium in a very perfect manner. The artificial beryl-glasses were made by taking the essential constituents of the mineral in the percentage proportions here given:—

Silica	67·5
Alumina	18·5
Glucina	14·0
	100·0

By the fusion of these ingredients together a beryl-glass is obtained which is practically identical in physical and chemical characters with that resulting from the fusion of the native emerald or beryl. In each case the hardness is reduced below 7 (that of quartz), and the specific gravity is only 2·42 instead of 2·7. These are signs that the compound has passed into the vitreous condition, and that it is destitute of crystalline structure. Though really identical in chemical composition with the native beryl, these beads are nothing more than "beryl-glasses," and can never in such a condition compete with the true native stones. Even were the exact tint of colour to be achieved, and if the hardness and specific gravity of the artificial glasses were much increased, still there would remain one fatal inferiority to the true emerald. For an emerald is dichroic, exhibiting, when viewed along the principal axis of the crystal, a green hue decidedly dashed with yellow, while when viewed in any direction transverse to this it shows a green colour verging a little towards blue. The instrument known as Haidinger's "dichroiscope" enables these two pencils, of differently coloured (and differently polarised) rays, to be seen alongside of each other if a beam of white light be transmitted through the crystal along one of the secondary axes. Now the crystalline structure upon which this dichroism depends has not yet been imitated in the artificial emerald, and in consequence the play or fluctuation of three hues—the normal emerald-green, the yellowish-green, and the bluish-green—of the natural crystal must be wanting in any vitreous imitation of it. This dichroism of the best emeralds is often seen when a number of small gems have been cut from a single crystal of apparently uniform colour; some of the polished pieces, having been cut in a different direction with regard to the axes of the crystal, will show decided variation in hue. The dichroism of other coloured beryls, as well as the emerald, is often well marked.

One of the main objects for which Greville Williams's research was undertaken was the settlement of the relation between the so-called organic matter found in the emerald and the colour of the stone. By an ingenious arrangement of apparatus, and by the use of the chromic acid method of burning the carbon and hydrogen present, this chemist has shown that colourless beryls sometimes contain carbon and hydrogen, just as the emerald does; and he has demonstrated that the carbon does not exist as a carbonate, though it may possibly occur in the form of microscopic diamonds. We are promised a further contribution to the chemistry of the emerald, with especial reference to the modes of separating and estimating the rare earth, glucina, which it—in common with the chrysoberyl, another mineral used in ornamental jewellery—also contains. The chemist Lœwy, who believed the colour of the emerald to be due to organic matter, detected the earth glucina, to the extent of ½ per cent, in the calcareous concretions which are associated with the matrix in which the mineral is found at Muzo, New Granada: pyrites, and the rare carbonate of the cerium metals known by the name of Parisite, also occur with the emerald. It would be as well to examine the Brazilian emerald for cerium and its allies.

The emerald is more prized now than formerly, as much as £40 having been given for a perfect emerald of 1 carat (0·20541 of a gramme, or 3·17 grains). An emerald of 1 carat, owing to its low specific gravity, is of course much larger than a sapphire of the same weight, and about one-third larger than a diamond. Large emeralds are seldom of uniform and deep colour, and hardly ever free from numerous cavities and flaws. In the Duke of Devonshire's collection there is a splendid crystal, about 2 inches in each direction, and weighing nearly 9 ounces troy, but it is much flawed. In the Green Vaults of Dresden there are some fine stones, while there are also numerous specimens in the Brazilian, Turkish, Portuguese, Austrian, Persian, and Papal Treasuries. Many of these have, however, been disfigured by perforations or inartistic engravings, and being in many cases merely rounded and polished, "*en cabochon*," are not capable of showing to full advantage those optical properties of dispersion, refraction, dichroism, &c., which may be developed by the judicious cutting of this gem. In the South Kensington Museum, among the rings of the Townshend collection, is a superb emerald, *step-cut*, and showing the characteristics of this stone in an eminent degree. The small so-called emerald of the Hope collection, in the same Museum, is nothing more than a piece of green glass. The true emerald is not well adapted for the purposes of the cameo and intaglio cutter, and was seldom engraved in Europe, although some examples exist,—as the Duke of Buccleuch's fine Medusa's head on emerald (a Florentine work of the seventeenth century). In Persia, India, &c., on the other hand, engraved or at least inscribed emeralds are not uncommon. But the transparent beryl and particularly the aquamarine have frequently been engraved with great skill and beautiful effect, both in classic and later times. Several good specimens of this kind are preserved in different public and private collections of gems, both in England and on the Continent. In the Bibliothèque Nationale of Paris there is a fine cameo of Julia, the daughter of Titus: the British Museum possesses some beautiful engravings on the same material, and there are others in the Museums of Florence, Naples, &c.

Some perfectly transparent and flawless aquamarines have been found of enormous size. One Brazilian stone weighs 225 troy ounces, and is as big as the head of a calf! The writer of this notice has seen one in London, of a rough spherical form, of perfect purity, though pale in tint, and nearly 4 inches in diameter. Mr. Hope possesses, in the hilt of Murat's sword, an aquamarine of remarkable size: there is also a fine richly-coloured stone amongst the Crown jewels in the Tower. In many private collections and in public galleries the different varieties of the beryl may be found in abundance: there is a good series of specimen crystals in the Mineralogical Department of the British Museum.

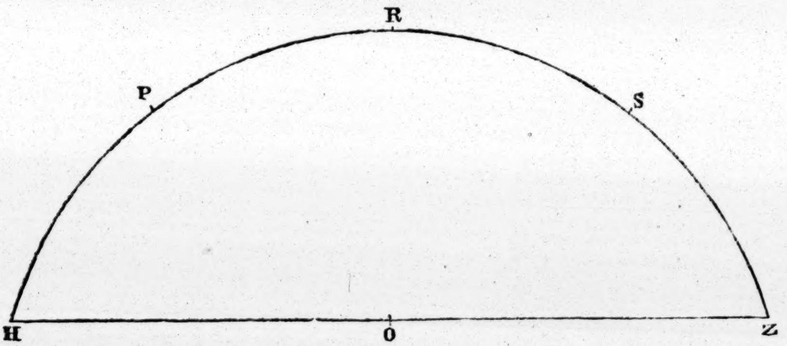
V. ON THE CURVED APPEARANCE OF COMETS' TAILS.

By Lieut.-Col. DRAYSON, R.A., F.R.A.S.

IN our paper on the Pole Star and Pointers in this Journal, we called attention to the fact that what would appear as a straight line to an observer in one part of the world, would appear as a curve or arch in the heavens to an observer on another part of the earth's surface.

As an illustration of this law, we called particular attention to the trace of the equinoctial on the sphere of the heavens, and pointed out how to an observer in middle latitudes the equinoctial would appear like an arch, cutting the east and west points of the horizon, and rising on the meridian to an altitude equal to the colatitude of the station on which the observer is located, whilst to the individual at the equator the trace of the equinoctial would appear as a *straight* line, rising from the east and west points straight to his zenith. Again we showed that to an observer at either pole the equinoctial traced in the heavens would coincide with the horizon, and would again appear as a straight line. It was also shown that a great circle passing from the east and west points of the horizon and through the pole would, for that part above the horizon and visible, appear as an arch to an observer in middle latitudes.

We will now suppose that three stars or any three celestial bodies (distant from the earth many million miles) were located on the equinoctial; that to an observer at the earth's equator these three stars, which we will call P, R, S, were so situated that the star R was at the zenith, the star P east, and the star S west of the observer, and both 45° above the horizon. These three stars being coincident with the equinoctial would appear in a straight line to the observer at the equator, and the line joining P R S would also appear as a straight line.



To an observer in, say, 51° north latitude, the equinoctial appears as an arch or curve in the heavens and not as a straight line, and all portions of the equinoctial appear as portions of an arch or curve, and therefore the line joining the three stars, P R S, would appear as an arch or curve to the observer in lat. 51°, though it would appear a straight line to an observer at the equator. The three stars on the equinoctial would appear curved to an observer in lat. 51°, as shown in the diagram, where H is the east, *z* the west points on the horizon, H P R S Z a portion of the equinoctial, and P R S three stars on the equinoctial. The line P R S will appear as a curve or arch.

We will now suppose that S is the head or nucleus of a comet, and that the tail extends along the equinoctial and as far as P. The comet's tail would now reach from P to R and S, and would appear as a curved line or arch to an observer in 51° latitude and to observers in all middle latitudes, but as a straight line to an observer at the equator, or to one at the North Pole. Consequently the curving or bending of a comet's tail is in this case an appearance only, due to the position of the observer, and the same comet's tail may appear at the same instant to two observers, one in middle latitudes and the other at the equator, as a curve and a straight line.

When the comet's tail is extended in that part of the heavens near the North Pole, a quarter rotation of the earth will produce similar effects to the preceding. As an example, suppose the head of a comet west of the pole and the tail coincident with the great circle passing from the west point on the horizon through the pole, then the tail being coincident with this great circle would appear curved.

In six hours the comet would be on the meridian and below the pole, and the tail being coincident with this meridian, would now appear as a straight line, whereas before it appeared as a curve. Thus the curvature of a comet's tail is an appearance only, and is due to the same law as that which causes the pointers to sometimes appear to point more directly towards the pole star than they do at other times.

During the year 1861 we happened to be staying in Hampshire with a friend who was a close observer of Nature. We were walking in a wood a few minutes before sunset, when happening to look at the sky through an opening in the trees, we remarked a bright object larger than Venus at her greatest brilliancy. The object we at once saw was in the north, and we therefore knew that it was no star or planet which had so brilliant an appearance, and before the sunlight had sufficiently decreased to show a tail, we had decided that a remarkable comet must have come suddenly upon us.

As the sun descended below the horizon and darkness stole over the scene, the comet's tail became visible, and stretched from the nucleus, which was below the pole and near the northern horizon to far up above the zenith and over towards the south. By means of a pocket sextant, which we had with us, we measured the length of the tail by measuring from the disc of the comet to those stars which could be seen within the tail. We found that 110° was the length of the tail, and we noted those stars which appeared to be in the centre line of the tail.

At about four o'clock in the morning our friend called us to come and look at the comet, which he announced had undergone a marvellous change.

When we first saw the comet and up to nearly midnight the tail appeared straight, and to point past the pole and up to the zenith; but at 4 A.M. the comet's tail had quite altered its shape, and, as our friend remarked, was more curved than a Turkish scimitar. Being aware at the time of those laws of curves and straight lines demonstrated in the preceding papers, we saw how good an example this comet afforded of the laws, for it was a practical illustration of a straight line becoming in appearance a curved line in consequence of six hours of diurnal rotation. We remarked that the comet's tail had not changed its position in the heavens as regards the fixed stars in the slightest degree, the very stars that were in the centre of the tail at 10 P.M. were also there at 4 AM.; and, in fact, we had the same problem exhibited in the heavens as we have endeavoured to demonstrate by diagrams in the preceding papers. The particular position occupied by the comet of 1861 was such that in six hours it changed the apparent form from a straight line to a curve, as seen by an observer in England, the comet's tail being directed from the northern horizon past the pole and zenith. If this comet instead of being so situated had its tail coincident with the equinoctial, it would never have appeared to us in England as a straight line, but always as a curve in the heavens during the whole time it was above the horizon. If, however, it had coincided with the equinoctial it would have appeared as a straight line to an observer at the equator of the earth, and never as a curve or arch. Thus, whether a comet's tail appears as a straight line or as a curve depends mainly on the position of the comet in the heavens and on the position of the observer on the earth. If a comet's tail were coincident with the equinoctial, and of, say, 80° length, it would appear to an observer in 45° N. lat. as a curve or arch. If this observer could be instantly transported to the equator, the comet would then appear as a straight line without any sign of curvature. If, again, the person were transported to 45° S. lat., the comet would again appear as a curve. A comet, therefore, that coincides with the equinoctial appears as a curve or a straight line, according as the observer is at one or another part of the earth, whereas a comet whose tail passes through the pole appears as a straight line or curve, according as the tail is directed towards the zenith or towards the east or west. Thus rotation in the last case produces the same effect as a different position of the observer produces in the case of a comet on or near the equinoctial. We need scarcely add that the same law holds good for nearly every part of the heavens.

The reader who has carefully read the preceding remarks will be enabled to state exactly those conditions under which the tail of a comet will appear a straight line or a curve.

It must be borne in mind that the fact of a comet's tail *appearing* as a curve or arch to an observer on earth does not in the least prove that the tail of the comet is actually curved ; it may be perfectly straight, and will yet appear as a curve from the reasons assigned.

There seems to have been some obscurity in the minds of former writers on astronomy on this subject of the curves of comets' tails, for we find the following statements relative to this curvature in the undermentioned works :—

"The tails of comets, too, are often somewhat curved, bending in general towards the region which the comet has left, as if moving somewhat more slowly, or as if resisted in their course."—*From Herschel's Outlines of Astronomy, Article* 557.

"The tails (of comets) appear to stream from that part of the nucleus which is furthest from the sun but seldom in the direction of a straight line joining the two bodies. They generally exhibit a sensible curvature."—*From Gallery of Nature, page* 112.

"P. Bienewitz, better known as Apian of Ingoldstadt, concluded from what he ascertained in the instance of the comet of 1531, that the tails are always opposite to the sun and in the direction of a line joining the centres of the sun and the comet; but more exact observation shows that as comets approach their perihelion the tails are gradually bent more or less towards the region they have left, as if there were a resisting medium. The tail of the comet of 1689 assumed the form of a Turkish scimitar ; and that of the comet of 1744 was bent like a quarter of a circle."—*From Smyth's Celestial Cycle, vol.* 1, *page* 221.

"If no comet ever exhibited any other than this peculiar form of tail straight and directed from the sun, we might frame an hypothesis which could account for the facts ; but in some instances there are many tails to the one nucleus, and these not straight but curved like a scimitar."—*From Popular Astronomy. By O. M. Mitchell, LL.D.*

It is interesting to find how much attention has thus been given to the fact that comets' tails are usually curved; but it seems that the geometrical laws which cause this curving have escaped observation. There is, however, one advantage derived from the problem, viz., that it appears it has prevented some hypotheses from being framed to account for what was *supposed* but *did not occur*.

It is most essential that before we invent a theory to account for what we see, we should be certain that we are not misled by mere appearances. We have demonstrated the cause of a comet's tail appearing in the heavens as a curve to one observer, whilst it appears at the same instant as a straight line to another observer at a different part of the earth; and we have shown that this appearance is due to the geometry of the sphere.

It seems singular to find that so many writers whose popular reputation in connection with astronomy is great have by their remarks on this subject shown they were quite unacquainted with what we may term the practical geometry of the sphere, and with those elementary laws of apparent curves and apparent straight lines in the heavens which we have demonstrated in connection with the pole star and pointers and the curvature of comets' tails. This singular neglect of so important a study must not be passed over without some further remarks, which we bring forward for the purpose of eliciting truth, and with the object of checking if possible an evil in connection with science which seems to be spreading greatly and rapidly. This evil is, that there are a very large number of persons who pass for and call themselves astronomers, who consider that this science, based as it is on observation, and regulated as it is by geometrical laws, is to be considered solely as a subject to be treated by physical theories, and to be explained by a sort of dogmatic assertion or sentence, which if it contain some such words as "gravity," "attraction," or "repulsion," is supposed to be unanswerable.

When we find that even in the newspapers men write and state that the principal object of expeditions connected with the transit of Venus is to enable navigators to better find their latitude and longitude at sea, we at once know the writer to be ignorant of his subject. When, again, we find critics speaking of the rate of rotation of a planet being dependent on the laws of gravitation, we again know them to have but little knowledge of what these laws can and cannot help us to discover. When, however, we find that a mere jumble of words, in which terms used in astronomy and in connection with physical science are mixed up together and put forward in a standard work intended for the guidance of the public as an explanation—when they have no real meaning as regards the facts they attempt to explain, we believe every true lover of science will agree with us that such proceedings should not only cease to be encouraged, but should be exposed and condemned.

We find also that the writers who thus attempt to explain by verbosity that which can only be solved by sound geometry almost invariably ignore facts. That the three angles of a plain triangle cannot be greater than 180° is to them of no consequence, and is to be overlooked if their not being greater interferes with some dogmatic theory. That these remarks are not uncalled for is, we believe, too well known to many really candid men of science. But that there should be no mistake on this point, we will now give one among many examples of the fact we wish to bring forward.

We have pointed out in this article that a comet's tail which coincided with the equinoctial would appear as a straight line or as a curve, according to the position of the observer on earth. We also demonstrated that a comet's tail which passed through the pole and was of considerable length would appear as a curve, and six hours afterwards as a straight line. The laws shown in these two cases hold good for the bending of comets' tails in all other parts of the heavens, and the *apparent* curvature is due to a geometrical law and not to any physical cause.

When then we find in a work of no less importance than the "Encyclopædia Britannica," under the article Astronomy, page 78, the following profound nonsense, put forward as an explanation of an effect due as we have demonstrated to a geometrical cause, it is evident how widely spread is this vicious system of explanation and how hollow and unsound are the scientific principles of those individuals who by such verbiage delude the ignorant and impose on the half scientific.

The following is the explanation given:—"The slight curvature which is generally observed (in a comet's tail) may be accounted for by combining the motion given to the vapours by the impulsion of the sun's rays with the motion of the comet in its orbit; for the detached vapours are driven by the impact of the luminous particles beyond the sphere of the comet's attraction, and consequently cease to follow the direction of the nucleus."

This explanation needs no further comment; it is but a specimen of a class of so-called science which is in the present day very extensively followed. Just as a savage who has once seen an electric telegraph will hope to explain everything from the effervescence of champagne to the moving of a locomotive by attributing these effects to electricity, so his mental type in England endeavour to explain each astronomical effect by attributing it to the laws of gravitation, to "impulsion," "attraction," &c., causes which, as in the present case, have often nothing whatever to do with the question under consideration.

Other important problems connected with astronomy are dependent on these same geometrical laws, and will be treated of at another opportunity.

NOTICES OF BOOKS.

The History of Music. Vol. I. From the Earliest Records to the Fall of the Roman Empire. By W. Chappell, F.S.A. London: Simpkin and Marshall. 1874.

Of the arts of the Ancients—of sculpture, painting, music—sculpture is best known to us; the others are almost unknown. A few late frescoes, a few late hymns to gods and goddesses,—these are all that we can place side by side with the grand sculptures of Pheidias and Myron, with those wonderful architectural works the Propylæa and the Parthenon, the very ruins of which impress us with wonder, and show how perfectly the harmonies of the eye were understood twenty-two centuries ago. But the poetical and dramatic art of the most cultivated of ancient nations has also been presented to us; in that marvellous trio, Æschylus, Sophocles, Euripides, we behold a combination or concentration of the purest and most perfect art. We know how the gods were addressed, how their mysteries were celebrated, how Antigone lamented, and how Edipus passed suddenly and silently from the eyes of men when the dread voice at Colonos cried "Come hither, come." But we know not how the players played, and the choruses sang at divine celebrations, or when the crocus-coloured peplos was carried to the summit of the Acropolis amidst the rejoicing of the people, and the rhythmic dances of maidens with golden grasshoppers in their hair.

We may, however, congratulate ourselves that we have in the work before us all that is known of Greek music, and much that has not been known before,—a Greek hymn to Nemesis harmonised by Macfarren, an explanation of Greek notation, Greek scales, and Greek singing; of *Harmonici* and *Kanonici*.

Histories of ancient music are not common, although, so long ago as 1581, Vincenzo Galilei published his "Dialogo della Musica Antica." The works best known to us in this country are those of Sir John Hawkins (1776) and Dr. Burney (1776—1789). The work of the former was cumbrous and unreadable, and met with but little favour, while Dr. Burney unfortunately relied to a great extent on Boethius for his knowledge of ancient music. But the treatise of Boethius was a broken reed, for its author utterly misunderstood Greek music, and concluded that the words *nete* and *hypate* (signifying respectively the shortest and the longest string of the seven-stringed lyre) referred to the top and bottom of the scale; thus he turned the Greek scale upside down. Yet the work of Boethius, "De Institutione Musica," was the text-book during the earlier Middle Ages, and till the time of Guido d'Arezzo, in the eleventh century.

The Greeks acquired their music from the Egyptians; *Renaissant* Europe acquired its music from Greek sources. "No Roman of antiquity," says Mr. Chappell, "is known to have made, or even to have attempted, any improvement in the science of music. The Romans received the diatonic scale, of tones and semitones, from the Greeks, at a time when it existed only in its primitive and imperfect form. Nevertheless they were content to retain it so, and did not follow the Greeks in any subsequent improvement. It is for that reason Greek music cannot be effectually learnt from Roman writers." In the fourth century the practice of singing alternate verses of psalms by a choir was introduced from Antioch, and this was called *antiphonal*. The introduction of Greek words in ecclesiastical music was another cause of confusion in the study of Greek music; for the meaning of *anti*, as applied to Greek music, is in the sense of *with* or *accompanying*, not *against*, and Greek antiphons were harmonious and concordant sounds, an octave apart. Again, according to Mr. Chappell, the Greek *Harmonia* means a "system of music," or simply "music," and has nothing to do with the modern word "harmony," either in its English, French, German, or Spanish sense. Also, the Greek *Melodia* does not mean melody according to our sense of the word. "Greek *Melos* had not necessarily any tune in it. It applied to the rising and falling sounds of the voice when linked together in speech, or in rhythm, as well as in music; so that recitation, without any musical intervals in it, would still be *melodia*. Thirdly, *Harmonike* does not mean *harmonic* or *harmonics*, but is a synonyme for *harmonia*. Again, *Sumphonia* does not mean 'symphony.' The last expresses our 'harmony,' viz., concords of notes of different pitch." No wonder that Greek music has puzzled so many modern writers; no wonder Dr. Burney calls it "a dark and difficult subject, which has foiled the most learned men of the two or three last centuries."

Our present musical scale is founded upon that of the Greeks; it is simply a re-adjustment of the Pythagorean scale, made in the second century A. D., by Claudius Ptolemy, the mathematician. The Greeks trace the origin of their music to Hermes, who invented the lyre. The old Homeric lyre was an instrument of four strings, and several accounts are given of its invention by Apollodorus, Diodorus Siculus, and others. According to a hymn to Hermes, of considerable antiquity, the god "found a mountain tortoise grazing near his grotto on Mount Kyllene. He disembowelled it, took its shell, and out of the back of the shell he formed the lyre. He cut two stalks of reed of equal length, and, boring the shell, he employed them as arms or sides to the lyre. He stretched the skin of an ox over the shell. It was perhaps the inner skin, to cover the open part, and thus to give it a sort of leather or parchment front. Then he tied cross-bars of reed to the arms, and attached seven strings of sheep-gut to the cross-bars. After that he tried the strings with a plectrum." According to another account, the lyre was invented by the Egyptian god Thoth, who, while walking along the shores of the Nile, happened to strike his foot against the shell of a dead tortoise, containing nothing within its shell but the dried cartilages of the animal. He was so pleased with the sounds produced that he constructed a musical instrument from the same shell, and put strings to it of dried animal sinews. It was with the lyre of Hermes that Orpheus charmed Creation, and even tamed the furies of Hades.

The earliest representations of musical instruments and of concerts are to be found among the frescoes and papyri-paintings of ancient Egypt. No less than thirteen different musical instruments have been noticed in various papyri:—Harp, lyre, lute, flute, double pipes, sistrum, &c. A conductor is often seen beating time with his hands; sometimes one conductor for the instruments, another for the chorus. Some of these representations go back as far as the time of Moses. "We may trace the prototype of every Greek instrument in Egypt. No kind of advance upon the music of that ancient country seems to have been made till the three Alexandrian mathematicians, Eratosthenes, Didymus, and Claudius Ptolemy, appeared successively upon the scene, and improved the scale. Eratosthenes, the first of them, was born about 276 B.C. He was director of the Alexandrian Library."

Pythagoras is believed to have imported the octave system from Egypt into Greece. We all know the old story of his having discovered it: when passing a blacksmith's shop he heard the consonances of the fourth, fifth, and octave, and weighed the hammers. The other story of the strings is less trite, but equally untrue. It is said that Pythagoras took strings of equal thickness and length, fixed them at one end, passed them over a bridge, and weighted them at the other end with weights of 6, 8, 9, and 12 pounds. But Galileo pointed out that in order to get the octave, the fourth, and the fifth, from such an arrangement, it would be necessary that the weights should be the squares of 6, 8, 9, 12. The doctrine of the Harmony of the Spheres was based upon the octave system of music. The earliest extant notice of this system is found in some fragments attributed to Philolaos, who was the first to make known many of the doctrines of Pythagoras.

Greek singing, according to Mr. Chappell, must often have severely strained the voice. In the "manly and severe" Dorian scale the key-note was the tenor *d* in the space below the treble clef. Aristotle says that few persons could sing the **νομοι 'ορφιοι**, addressed to Apollo, on account of their high notes. "Apollo seems to have been addressed as if he had been troubled with deafness, or was supposed to be a long way off; and perhaps that was the general style of Greek antiquity. It recalls Elijah's mockery of the priests of Baal—telling them to 'cry aloud: peradventure he sleepeth, and must be awakened.'" But for ordinary purposes the Greek compass was much the same as that of the present day. The various Greek octaves were called Lydian, Phrygian, Dorian, &c., by Euclid, Gaudentius, and other writers.

Any time during the last two centuries the question whether the Greeks practised simultaneous consonances mixed with discords (what we call *harmony*) has been discussed. It was discovered that ‘Αρμονι’α does not mean simultaneous concordant sounds: συμφωνια is the word for simultaneous consonance, but this was generally not allowed, "If the enquiry had been pursued in the only proper way, by searching for and comparing Greek definitions of *harmonia*, its meaning would inevitably have been traced to the theory and practice of music, and identical with the later word Harmonike. *Harmonia* includes poetry united with music, but not poetry alone, and so it has a more restricted sense than Mousike. Again, the chanting of poetry, though unregulated by musical intervals, is *melodia*, and the metre of poetry brings it within the denomination of Mousike; but it is not *harmonia*. So that the primary translation of the word *harmonia* is our 'music.'" Mr. Chappell quotes various ancient authors to prove that harmony was well known to and employed by the Greeks. He quotes, among other authors, Seneca and Cicero: the former says—"And now to music: you teach how voices high and low make harmony together, how concord may arise from strings of varying sounds." (Is not this rather a free translation Mr. Chappell? "*Doces me quomodo inter se acutæ et graves voces consonent, quomodo nervorum disparum reddentium sonum fiat concordia.*") "Teach rather how my mind can be in concord with itself, and my thoughts be free from discord." Cicero, in the second book of the "Republic," says—"For as in strings or pipes, or in vocal music, a certain consonance is to be maintained out of different sounds, which, if changed or made discrepant, educated ears cannot endure; and as this consonance, arising from the control of dissimilar voices, is yet proved to be concordant and agreeing, —so, out of the highest, the lowest, the middle, and the intermediate orders of men, as in sounds, the State becomes of accord through the controlled relation, and by the agreement of dissimilar ranks; and that which, in music, is by musicians called *harmony*, the same is concord in a State." Mr. Chappell considers that Cicero's mere definition of the word *concentus*, and the assertion of Aristotle that "all concordant sounds are more agreeable than single notes, and of concords the octave is the most agreeable," are alone sufficient to prove that the Greeks were acquainted with harmony. "But," he adds very happily, "floating upon the surface of music has been for ages more popular than diving."

We come now to the eighth chapter of the "History," which is important, inasmuch as it contains three Greek hymns, written out in accordance with our modern notation, and supposed to be the only trustworthy remains of Greek music. They were first published by Vincenzo Galilei, the father of the astronomer, in 1581, and were copied from Greek MS. in the library of Cardinal St. Angelo. The date of the hymns is uncertain, but it is not considered probable that they are older than from the second to the fourth century, A. D. The first is a hymn to Calliope, with an accompaniment in the Hypo-Lydian mode by George Macfarren; the second a hymn to Apollo, both grave and solemn, and something like some of the graver hymns of the Catholic Church; the third, a hymn to Nemesis, is by far the most striking of the three, and contains a very definite and distinctive theme.

A curious example of the delicacy of the human ear is to be found in the fact that the so-called "Comma of Didymus," the interval between a major and a minor tone, or between the 80th and 81st part of a vibrating string, sufficed to produce the great change between the scale of Pythagoras and the scale which we now use. The harmonic scale was developed during the last century; it was discovered in 1673, by William Noble, of Merton College, and Thomas Pigot, of Wadham. It is the scale of natural sounds arising from the successive aliquot divisions of a string, and, according to Mr. Chappell, it forms the basis of the science of music.

The last four chapters of Mr. Chappell's book are devoted to an account of the musical instruments of the ancients:—Various kinds of pipes, and the way in which they were sounded, and the materials of which they were made—lotus, laurel, palm-wood, pine-wood, box-wood, beech-wood, elder-wood, ivory, reeds of various kinds, leg-bones of animals and of large birds (such as the eagle, vulture, and kite). Then there were pipes receiving their name from the country in which they were invented—as Alexandrian, Tuscan, Theban, Scythian, Phœnician, Lybian, Arabian, Phrygian. The Theban pipes were made of the thigh-bones of a fawn, and were covered with metal. "The length of Arabian pipes was proverbial, and a man of whose tongue there seemed to be no end was called an Arabian piper." The "sweet monaulos" was for weddings, and the Phrygian pipes were "for wailing and lamenting." After wind instruments come instruments of percussion, such as the drum, dulcimer, timbrel, sistrum, cymbals, and krotala. According to Mr. Chappell the sistrum was simply shaken, so as to produce a jingling sound; Plutarch has given a long account of the instrument. The Assyrians appear to have had a sistrum of a different form, which was struck by a rod of metal serving as a plectrum.

The Greeks appear to have possessed three kinds of cymbals. As for stringed instruments, their number was considerable: there were many kinds of lyre, harps, psalteries, four-stringed trigons, and guitar-like instruments. The largest kind of lyre had many strings, and was placed upon a stand; the others were carried in the hand. Athenæus quotes a singular story told by Artemon, to the effect "that Pythagoras once strung the three sides of a Delphian tripod, such as was used to support an ornamental vase, and that he tuned one side to the Dorian scale, another to the Phrygian, and the third to the Lydian scale or mode. So far all was possible, but it is improbable that Pythagoras should have attempted it, because there could be no tone from such a tripod, for it had no sounding-board. The minuteness of the remaining part of the story proves the whole to be a myth. Artemon adds that Pythagoras contrived a pedal to turn this tripod, and that he twisted it about with such rapidity while he was playing that any one might have fancied he was hearing three players upon three different instruments."

Mr. Chappell considers the Greeks to have been very uninventive in regard to musical instruments, which all seem to be Asiatic or Egyptian. The lyre is found in Egyptian paintings before the Greeks existed as a nation. "We can find no new principle for stringed instruments discovered by a Greek, nor anything new in pipes. All was ready-made for them, together with their system of music. The Greeks were even inapt pupils; for, although they had many strings ever before their eyes, they did but reduce the number, after a time, to bring the instruments down to their own level. They practised a certain amount of harmony, but not so much as earlier nations." Our author even goes so far as to compare Greek music of an early date with modern Japanese music, of which a curious account is given on p. 304. We are glad to notice representations (pp. 314—315) of the beautiful harps from the tomb of Rameses the Third.

The thirteenth and final chapter of the work is devoted to organs, and Mr. Chappell has carefully investigated the various forms of hydraulic and other organs described by Hero in his *Πνευματικα*. He has even had a model of one of them constructed, and points out its advantages. The invention of the hydraulic organ is attributed to Ctesibius, who appears to have lived about 284 B.C. The precise meaning of the word *οργανον* has sometimes led to confusion. It was often used to mean any *instrument;* it might be a surgical instrument, or a musical instrument, or an organ of sense,—as the instrument of reasoning,—and so on. The first description of the hydraulic organ is given by Hero of Alexandria, who was a pupil of Ctesibius. A second full account of it is given by Vitruvius, in his Treatise on Architecture, written between B.C. 20 and 11. Many of the early editions of the "Pneumatics" appeared with drawings, but none of these are said to be older than the fourteenth or fifteenth century. A capital drawing of the organ is given on p. 340. It had keys, pipes, valves, a wind-chest, and a pump for condensing air into the wind-chest, and even stops. The Romans, in the time of Nero, appear to have had organ-contests, and medals were given as prizes, one of which is in the British Museum.

Mr. Chappell's work is altogether interesting, and contains a good deal of new matter. A few of the chapters appear to be unnecessarily complex, and perhaps can be somewhat simplified in a second edition. A few errors exist, which may also then be altered: perhaps the most important of these is to be found in the foot-note to p. 187-188, in which Lissajous' Figures are wrongly described as produced by sand on vibrating plates. The book is well printed and illustrated, and we shall look forward with interest to the appearance of the remaining volumes.

The Harveian Oration for 1874. By CHARLES WEST, M.D., F.R.C.P. London: Longmans, Green, and Co. 1874.

THE practice of giving the annual Harveian Oration in Latin has been discontinued for several years; and in this country it is not easy to hear a Latin address, save on certain occasions in our older Universities. In Leyden, and in some of the German and Italian Universities, it is, however, a far commoner practice. The present oration could well be converted into somewhat florid and verbose Latin: for example, think of such resounding periods as the following:—"When your commands, Sir, were first laid upon me to undertake this most honourable, most arduous office, I studied, as a preparation for its accomplishment, all the Harveian orations that I could meet with." Or, again—"Such was the man, such were his pursuits; loving knowledge for its own sake; loving it, too, not in pride of intellect," &c. The oration relates to the Life and Times of Harvey. It tells us that he was born in 1578, that he left school and entered at Caius College at the age of 15, and took his B.A. in 1597. On leaving Cambridge he went to Padua, then a most celebrated University and Medical School. At one time it possessed no less than 18,000 students, and it numbered among its Professors some of the most eminent men in Europe. Harvey passed nearly five years at Padua, and took his degree in 1602; on returning to England, he became M.D. of Cambridge in 1603, F.R.C.P. in 1607, married and commenced practice in London, and in 1609 became Physician to St. Bartholomew's Hospital, and then he worked hard at anatomical discoveries. His greatest discovery is described by Dr. West as follows:—"*First.* After corroborating the statements of those who had denied either that blood transudes through the walls of the ventricles, or that the pulmonary veins bring back to the left side of the heart air commingled with the blood, he asserts that the left ventricle has no other function than that of impelling the blood brought to it through the arteries, which themselves contain blood and nothing else, not air, nor vital spirit, but blood purified by its passage through the lungs, and so made apt for the nourishment of the whole body. And *second*. That while the arteries thus distribute everywhere the fresh pure blood, the veins with which they communicate bring back that same blood, no longer pure, to the right side of the heart, whence it is once more transmitted to the lungs, thence carried again revivified to the left ventricle, and then once more distributed throughout the body, its changes not being those of an ebbing and a flowing tide, but the ceaseless current of an onward rushing river." The doctrine of the circulation of the blood was taught as early as 1615 by Harvey, but it was not till the year 1628 that he published his "Exercitatio de Motu Cordis." The conclusion of the oration is devoted to a somewhat graceful allusion to benefactors of the College:—"Benefactors—those who have done us good, or have shown us kindness; in vulgar sort, those to whom we owe gifts of money, grants of land, something or other that can be bought or sold in open market. According to this reading, few indeed have been our benefactors." All the money properties of the College do not exceed £600 a year, and the plate not more than £20. The College, says Dr. West, will bear no comparison with the magnificent Halls of the City Companies; and then later on he says—"Our great benefactors are they who have left us the inheritance of their example." Such as Sydenham, and Meade, Jenner, and Bright, and pre-eminently Harvey.

Divine Revelation, or Pseudo-Science? An Essay. By R. G. Suckling Brown, B.D. London: Longmans, Green, and Co. 1874.

This is one of the many anti-Darwinian books, which the author calls "our form of resisting the hypothesis of evolution, and kindred pseudo-sciences." We fear we must pronounce Mr. Brown's book to be illogical and unscientific, and of little value even to the opponents of the Darwinian theory. We give the concluding lines, and so leave the book to the judgment of the reader:—"The admirable adaptation of the bird's wing; the contrivance of the human fore-arm, hand, and foot; the poise of suns and planets in infinite space: their accelerated and diminished velocities in the ratio of their distance from their centres of attraction or gravitation; the apparatus of the infusorial *Rotifer*, the tyrant over the inhabitants of a drop of water; the delivery of its contemplated victim by its provided apparatus for withstanding the torrent, stirred by its tyrant’s paddles; all these great, minute, and wondrous; all these most curious and admirable works of an Almighty Designer and Creator, who governs by compensation, and tempers compensation by instituting a maximum of enjoyment through a minimum of suffering; who maintains His creations by the law of self-preservation: all these, and multitudes of other benignant forethoughts, these busy physiologists overlook, neither thinking, nor asking, nor, they give us reason to imagine, caring to know ‘Whose works are these?’ Nay, more! while they are loquacious of acids and salines, they forget to think out or to ask ‘Whose works are these?’ Who made acids and salines? And while they trace ‘*the human animal*,’ for such they account man to be, and such they would make him, to the askidion or the monad, they consider not, and forget to ask, ‘Who made the monad and the askidion?’”

The Correlation of Physical Forces. Sixth Edition. With other Contributions to Science. By the Hon. Sir W. R. GROVE, M.A., F.R.S., one of the Judges of the Court of Common Pleas. London: Longmans, Green, and Co. 1874.

WE are very glad to welcome a sixth and revised edition of Sir W. Grove’s celebrated Essay. By “revised” we mean simply that those discoveries in Science which bear upon the subject of the relationship of the physical forces made since the first appearance of the Essay, in 1843, have been added, and the whole has been thus rendered more complete. It may be said that the Essay has done its work, that scientific men fully realise the correlation of the physical forces, which has been so wonderfully exemplified during the last thirty years by the science of thermodynamics, but the Essay still remains a model of accurate reasoning, and of an elegant scholarly style, and will always be studied with advantage by the student of Science. We need not give a very detailed account of its object; it is too well known to require that. But, in brief, the design is to show that if we take the so-called physical forces,—motion, light, heat, electricity, magnetism, and chemical affinity,—each one is capable of producing the remaining five; and, further, from first to last the author combats the idea of such hypothetical entities as ethers, subtle fluids, &c., which have been devised to account for phenomena otherwise not easily explained. He endeavours to show that each and all of the physical forces is an affection of matter, not matter itself,—that, in fact, it is a mode of motion; and this idea, now fully admitted in the case of heat and light, will, no doubt, in our generation be extended to electricity and magnetism.

Without giving any very connected chain of reasoning in a book which has long been before the public, we will indicate here those passages which are striking from their originality or precision, or from some other cause. As to causation, our author remarks that the view entertained of it is usually that of Hume, who refers it to "invariable antecedence," a cause being that which invariably precedes, and in effect that which invariably succeeds; and he shows that this definition will not bear strict scrutiny. As to the aims of physical science, he remarks—"Instead of regarding the proper object of physical science as a search after essential causes, I believe it ought to be, and must be, a search after facts and relations; that although the word Cause may be used in a secondary and concrete sense, as meaning antecedent forces, yet in an abstract sense it is totally inapplicable."

Form is defined as "that active principle inseparable from matter which is supposed to induce its various changes,"—not, to our mind, a satisfactory definition, although we know not where to look for better. Referring to "force in abeyance," or tension, or what we should now call "potential energy," Sir W. Grove gives us the following comprehensive and perfectly logical remarks:—"But it may be objected, if tension or static force be thus motion in abeyance, there is at all times a large amount of dynamical action subtracted from the universe. Every stone raised and left upon a hill, every spring that is bent and has required force to upraise and bend it, has for a time, and possibly for ever, withdrawn this force, and annihilated it. Not so; when we raise a weight and leave it at the point to which it has been elevated, we have changed the centre of gravity of the earth, and consequently the earth's position with reference to the sun, planets, and stars; the effort we have made pervades and shakes the universe; nor can we present to the mind any exercise of force which is not thus permanent in its dynamical effects. If, instead of one weight being raised, we raise two weights, each placed at points of the earth diametrically opposite to each other, it would be said, here we have compensation, a balance, no change in the centre of gravity of the earth; but we have increased the mean diameter of the earth, and a perturbation of our planet, and of all other celestial bodies, necessarily ensues."

The definition of heat as "a communicable molecular repulsive force" would scarcely be very generally accepted, we think. A repulsive force cannot readily be defined, or in the abstract be conceived; but if we regard heat as vibratory motion, we can at once realise how expansions are produced by the addition of such motion to congeries of molecules, which vibratory motion will of necessity drive them further apart.

Of electricity, Sir W. Grove says—"I think I shall not be unsupported by many who have attentively studied electrical phenomena, in viewing them as resulting not from the action of a fluid or fluids, but as a molecular polarisation of ordinary matter, or as matter acting by attraction and repulsion in a definite direction."

The remarks to be found on pp. 106 and 107, relating to the finite nature of our intellects, are much to be commended to the notice of scientific men, at a time when perhaps as much as in that of the Greek Positivists, or more modern Encyclopedists, intellectual pride is the bane of too many otherwise great minds:—"Men are too apt," remarks our author, "because they are men, because their existence is the one thing of all importance to themselves, to frame schemes of the universe as though it was formed for man alone: painted by an artist of the Sun, a man might not represent so prominent an object of creation as he does when represented by his own pencil."

Sir W. Grove often leads us from a purely physical to the verge of metaphysical discussion. In regard to the boundaries of the universe, he says—"We cannot conceive a physical boundary, for then immediately comes the question, What bounds the boundary? and to suppose the stellar universe to be bounded by infinite space, or by infinite chaos, that is to say, to suppose a spot—for it would then become so—of matter in definite forms, with definite forces, and probably teeming with definite organic beings, plunged in a universe of nothing, is to my mind, at least, far more unphilosophical than to suppose a boundless universe of matter existing in forms and actions more or less analogous to those which, as far as our examination goes, pervades space."

The last of the physical forces which is considered is chemical affinity, of which it is truly observed that it is that mode of force of which the human mind has hitherto formed the least definite idea. We think that the weakest part of this Essay is that in which the other physical forces are said to produce chemical affinity: thus we read on p. 26—"In the decompositions and compositions which the terminal points proceeding from the conductors of an electrical machine develope when immersed in different chemical media, we get the production of chemical affinity by electricity, of which motion is the initial source." Again, light may effect chemical combinations and decompositions, but it does not *produce* chemical affinity as a force; it simply determines a chemical change, or determines a manifestation of chemical force, which had all along been existent in the bodies which undergo the change. We find, among the concluding remarks, a statement of instances in which some one force being disturbed, all the others appear:—"Thus, when a substance, such as sulphuret of antimony, is electrified, at the instant of electrisation it becomes *magnetic* in directing at right angles to the lines of electric force; at the same time it becomes *heated* to an extent greater or less, according to the intensity of the electric force. If this intensity be exalted to a certain point the sulphuret becomes luminous, or *light* is produced: it expands consequently, *motion* is produced, and it is decomposed, therefore *chemical action* is produced." This is a kind of *tutti*, after each one has been singing his solo.

Few words are used more loosely and indefinitely than the word *Nature;* it is sometimes called "the principle which produces all things;" hence, poetically, the "Universal Mother," "Madre Natura," &c. We remember to have read, in one of the older numbers of the "Philosophical Transactions,"—"By Nature I understand the works of God manifested in Creation;" then we talk about the "laws of Nature," the "works of Nature," &c. Sir W. Grove says (p. 152)—"The word 'Nature' is still more personified; instead of being used to denote, what alone it can denote,—namely, things as we see, hear, or feel them, and their relations ascertained by comparison and abstraction,—Nature is treated as a sort of Superintending Angel, who enjoins this, permits that, and forbids the other." Finally, as to matter and force, he says—"The evidence we acquire of the continued existence of matter is by the continued exertion of the force it exercises, as, when we weigh it, our evidence of force is the matter it acts upon. Thus matter and force are correlates in the strictest sense of the word; the conception of the existence of the one involves the conception of the existence of the other: the quantity of matter, again, and the degree of force, involve conceptions of space and time."

The Address "On Continuity," which has already reached a third edition, was delivered by Sir W. Grove, as President of the British Association, at Nottingham, in 1866. It is, to our mind, quite a model of what a President's Address should be. Not a vehicle for the exclusive advocacy of Darwinism and free thought, but a *resumé* of the whole aspect of Science at that particular time, and an allusion to all the more dominant phases of thought which then prevail, and to the more prominent discoveries and generalisations. These are interspersed with his own far-seeing and luciferous ideas:—"As Phlogiston," he says, "and similar creations of the mind have passed away, so with hypothetic fluids, imponderable matters, specific ethers, and other inventions of entities, made to vary according to the requirements of the theorist, I believe the day is approaching when these will be dispensed with, and when the two fundamental conceptions of matter and motion will be found sufficient to explain physical phenomena."

The remainder of the work (that is to say, half of it) contains Sir W. Grove's researches in experimental science. Commencing with the nitric acid battery, now so well known and so much used, and which was devised in 1839, we find, in succession, the gas battery, the decomposition of water by heat, the striæ in the electrical discharge, and the effects of heat on fluids. There are many other papers, all containing a good deal of suggestive matter, and hints as to new researches.

We know of no book, save the admirable "Experimental Researches" and "Chemical Manipulation" of Faraday, which we could more recommend, both to the general reader of Science and to the young student, than this: clear and elegant in style, powerful in grasp, close and precise in reasoning, eminently suggestive, and very comprehensive, the book will continue to be a standard scientific work, and will always find a welcome place among the archives of the History of Scientific Ideas.

The Universe and the Coming Transits. Presenting Researches into, and New Views respecting, the Constitution of the Heavens; together with an Investigation of the Conditions of the Coming Transits of Venus. Recently confirmed by a Unanimous Vote of the Chief Astronomers of Great Britain. By RICHARD A. PROCTOR, B.A. (Camb.). London: Longmans, Green, and Co. 1874.

MR. PROCTOR is certainly the most prolific scientific writer of the age; he is a very Lope de Vega among scientists: book after book appears from his pen; his thoughts fly out as the sparks fly upwards. Sun, moon, and stars have alike received detail treatment at his hands; and here we have a new theory of the universe, and a collection of all the matter relating to the transit of Venus which he has published in the "Journal of the Royal Astronomical Society," and elsewhere. The book, in common with all his works, is very profusely illustrated; maps, charts, plans, woodcuts, are scattered throughout the volume,—for Mr. Proctor is a good draughtsman as well as a learned astronomer.

The first Essay, "On Star-Streams," treats of the Milky Way, which is now believed to consist of myriads of suns, around which no doubt there are attendant planets, which latter may contain countless numbers of living creatures. A very interesting account is given of the changes in the appearance and position of the Milky Way at the same hour, in different seasons of the year. By a simple illustration of the appearance that the lights of a large town would present to anyone looking from a height, we are shown that we must not necessarily suppose that the brilliancy of a star is positive evidence of *proximity*, but rather must bear in mind that we are dealing with a sphere *full of stars*, distributed through space, some at much greater distances from us than others, and *not* with a spherical surface *covered* with stars, as represented by a globe or map. For aught we know the whole stellar region may be occupied, more or less richly, by a variety of forms of matter other than stars or suns, and nothing but the most patient and systematic analysis can ever lead to the solution of the great problem of star-depths.

The article entitled "A New Theory of the Universe" is at once the longest and the most interesting contained in the volume: it was originally brought out in "The Student" for February, March, and April, 1869; therefore many discoveries and some extensions of the subject have been made since, but this in no way interferes with the correctness of the paper. Mr. Proctor begins by a comparison of what was known of the solar system at the end of the last century with what is known at the present time: first among the remarkable discoveries made within the last sixty years must be noticed the great increase in the number of primary attendants upon the sun; the 98th asteroid was just discovered at the time this Essay was written, and yet it was but on the opening day of the present century that the first of these bodies was brought to light. The mind gets bewildered in imagining all these bodies, primaries of the planetary system, revolving in paths closely interwoven, the more so when—given these constant additions to the numbers known—we may fairly assume that for each discovered asteroid there are to be reckoned tens, perhaps hundreds, which will never be found out. Then the myriads of dependent comets that are found to exist within the solar system, revolving round the great centre in the most eccentric orbits, must still remain among the mysteries of Science. At the present time, when the minds of even the most unscientific among us have been more or less interested by the appearance of a comet, this branch of the subject presents a special attraction, and it is most ably treated. A still more remarkable feature of modern astronomical discovery remains to be noticed; that is, that meteors, shooting stars, and aërolites, which have always been regarded as meteorological phenomena, must now be placed among the attendants of the sun. It is beyond doubt that the earth encounters fifty-six systems at least of these small bodies, and "What is the likelihood," asks Mr, Proctor, "that if there were only a few hundreds of such systems the earth would encounter so many as fifty-six?" The extreme probability, nay certainty, is that such systems may be reckoned not by hundreds and thousands, therefore, but by millions of millions. A close connection, if not identity, can be traced between comets and shooting star systems, as it is discovered that at least two of the meteoric systems coincide with the orbits of known comets. The Sidereal System next engages the attention, and the last twenty pages of this Essay are devoted to the consideration of what our astronomer terms "those mysteries of mysteries," the nebulæ. The entire paper deserves careful and attentive study.

The next article, bearing the title "What Fills the Star Depths?" is almost entirely devoted to mathematical problems proving that the arrangements of stars in different regions, &c., is not the result of accident, but rather that some real laws of aggregation exist among the stars. And then we come to an Essay of great interest, headed "Star-Drift," and treating—as the name implies—of stellar motion, particularly of the motion of the "sun, with his whole *cortege* of planets and cometary systems sweeping swiftly through space." This star-drift, however, must not be confounded with the phenomenon of drift as applied to Ursa Major and other constellations, and treated of in another part of the same paper. The star-drift due to the sun's motion in space may be called *general*, and has altogether a different significance to the other star-drift, which is only *local*. The one takes place in almost exactly the reverse direction from the other, and is perfectly distinct. In this chapter Mr. Proctor more than redeems a little self-assertion observable elsewhere, by his cordial expressions of admiration of the labours of his great countrymen, the Herschels.

The following Essays, "Are there any Fixed Stars?" "News from the Stars," "On two Rich Nebular Regions," &c., are reprinted from various sources, and discuss the most recent problems in Astronomy. The Essay "On the Construction of the Heavens," containing Jean Paul's wonderful dream of the universe, appeared in this Journal in July, 1872.

The second part of the volume—about one-third—treats exclusively of the coming transits of Venus, and is more technical, and consequently less popularly interesting, than the former portion of the work. The first transit will take place on December 8th of the present year; the second on December 6th, 1882; and both have such important bearing upon the problem of the sun's distance that men of science are anxiously looking forward to the times of their occurrence. After the present century no like phenomenon will occur until the year 2004; and in one respect the transit now at hand will present even better opportunity for making important calculations than that of 1882, so that for a number of years astronomers will be without the means of remedying any omissions now made: it is this consideration that induces Mr. Proctor to appeal most earnestly to his country to send out expeditions suitably furnished for making the most effective observations for the determination of the sun's distance: he feels that the scientific honour of his country is at stake, and most clearly and ably shows the manner in which it may be saved. There seem to be difficulties in the way of carrying out many of these observations, but it is greatly to be desired that the subject will meet with the recognition it deserves: any omission in making the proper arrangements for this important work might be obviated by due attention to the details so clearly set forth by Mr. Proctor; and we cannot close his book without a feeling of admiration for the manner in which he has thought well to bring his researches thus before the public, and for his perseverance and industry.

Handbook of Natural Philosophy. By Dionysius Lardner, D.C.L. Hydrostatics and Pneumatics. Edited and the greater part re-written by Benjamin Loewy, F.R.A.S. London: Lockwood and Co.

In sciences like Hydrostatics and Pneumatics, on whose facts there is little, if any, variety of opinion, and which have been undisturbed by any recent revolution in theory, or even in nomenclature, the compilation of handbooks is comparatively simple, and there is accordingly very little scope for criticism. The work before us is well arranged and clearly written, and may be recommended to all who require a knowledge of the subjects treated of without having the time or the inclination to enter upon mathematical refinements. Purists in methodology might, perhaps, raise the question whether the consideration of the diffusion of liquids, of the division of soluble bodies into crystalloids and colloids, and of the phenomena of osmose, can be legitimately introduced into a treatise on hydrostatics, involving, as they do, chemical principles.

Plattner's Manual of Qualitative and Quantitative Analysis with the Blowpipe. Revised and enlarged by Prof. Th. Richter. Translated by H. B. Cornwall, assisted by J. H. Caswell. Second Edition, revised. New York: D. Van Nostrand.

Prof. Plattner is generally recognised as *facile princeps* among authorities on blowpipe analysis, and his great work has passed beyond the domain of criticism. Of all the editions which have appeared in the English language, the present must claim the palm. It is founded upon the most recent German edition, as enlarged and revised in accord with the latest discoveries by Prof. Richter, of the Freyberg Mining School, the worthy pupil of Plattner. The translation has been carefully and judiciously executed. We are happy to say that there has been no attempt at abridgment, a thing impracticable without sacrificing that thoroughness which was a characteristic of Prof. Plattner, and on which the main value of his work depends.

We hope that this edition may be duly appreciated, and consider that it cannot fail to lead to a wider knowledge of the utility and the resources of the blowpipe.

Theory of Arches. By Prof. W. Allan. New York: D. Van Nostrand.

This work forms one of the "Science Series" of the eminent publisher by whom it is issued. It is an expansion of the views of the late Prof. Rankine upon this subject. The writer develops the theory of arches by beginning with a consideration of forces acting upon a suspended chain or cord. We have no doubt that it will prove of great value to architects and engineers.

Geology. By T. G. Bonney, M.A., F.G.S. London: Society for Promoting Christian Knowledge.

The author of this little treatise has, as he tells us in the Preface, "attempted to set down briefly the principal facts of Geology, and the conclusions which have been drawn from them; to indicate the nature of the earth's crust, the processes which have acted and are still acting upon it, and the probable history of that little portion upon which we live." The attempt has been by no means unsuccessful, and the result is a work well calculated, we believe, to lead to further enquiry. With the following remark we most heartily concur:—"The great aim of the Natural Sciences is to teach students to observe and think for themselves: when this result is not produced they are mere cram, and do more harm than good."

The wondrous tale written on the rocks is told here in the simplest possible words, and technical language, if not entirely avoided, is kept subordinate. The author, we should say, is not one of those men of science who would write a book merely for the sake of displaying a "fire-new" nomenclature. The very appearance of this work, and the frank admission not only of the incalculable age of the world we inhabit, but of the antiquity of the human race, is an instructive sign of the times. Had such a work been issued, under such auspices, half a century ago, certain of our contemporaries would—if such a thing be possible—have out-scolded and out-shrieked Louis Veuillot and Joseph de Maistre.

We wish this book a wide circulation, and feel convinced that it will have a good effect on the public mind.

Fourth Annual Report of the Board of Commissioners of Public Charities of the State of Pennsylvania. Harrisburg: B. Singerly.

This report contains much interesting information on the management of reformatories, criminal schools, and lunatic asylums in Pennsylvania. The treatment of pauper lunatics in some of these establishments appears to be shocking (cxlvii.). It seems from the "suggestions," p. 105, that in America persons who have been so unfortunate as to witness the commission of a crime may be committed to prison—if unable to find security for their appearance—in order to secure their evidence at an approaching or postponed trial. Apart from the obvious injustice of such a system, we think it must frustrate the enforcement of the law, by furnishing those who are in any way aware of the perpetration of a crime with a reason for keeping silence even more potent than the insolence and the brow-beating which witnesses, except persons of rank and station, have to suffer from counsel in England.

To statisticians, philanthropists, and persons interested in the management of asylums, hospitals, gaols, and poor-houses, this volume will prove invaluable.

Mineralogy. By FRANK RUTLEY, F.G.S. London: T. Murby.

THIS work belongs to "Murby's Science and Art Department Series of Text-Books," edited by B. J. Skertchly, F.G.S. What share, if any, the last-named gentleman has had in the production of the treatise, or to what extent he is responsible for its contents, it does not appear. The work, as we are told in the Preface, is destined to meet "the requirements both of elementary and advanced students for the Science and Art Department Examinations." It may, perhaps, be looked upon as an old-fashioned prejudice, but we like people to study any subject whatever in order to know; and not in order to pass examinations. We dislike cram, crammers, and crammees. We have no sympathy for the man into whom a certain dose of science has been forced at high pressure, like carbonic acid into a bottle of soda-water. When the pressure is removed, the science in the one case and the gas in the other escape with noise and effervescence, leaving a residue stale, flat, and unprofitable. But whilst thus taking exception to the declared object of the work, we must speak favourably of its contents. It might prove a difficult task to present a greater amount of useful matter in so limited a compass. The distinctive features to which the author lays claim are the general arrangement of the materials, the minerals being "grouped according to their most prominent basic constituents, and preceded by a brief description of the chemical and physical characters of the leading base in each group."

In the illustrations there is a novel feature:—The manner in which crystals are drawn and their faces shaded is peculiar, the shading or stippling being made to show at a glance the mutual relation of the faces developed on compound forms. The classification of the silicates has been made to depend on the crystallographic systems to which they belong.

The work begins with a definition of minerals as distinguished from organic bodies, and an explanation of the difference between mineral species and rocks, showing the respective spheres of the geologist, the mineralogist, and the chemist. We find next a condensed account of chemical composition, of the elements, of acids, bases, and salts, and of isomorphism. The second chapter teaches—as far as it can be attempted in a compass so brief—the use of the blowpipe. The physical properties of minerals form the subject of the next two chapters. To this succeeds an account of crystallography, followed by a systematic description of mineral species, forming the main portion of the work. The sources and occurrence of the various minerals are, for the most part, carefully given, though such localities as "Siberia," "Australia," and "United States," must be considered rather vague. Precision in this matter is of great importance in every department of Natural History. Even in case of widely distributed forms it is safest to state the exact spot where they have been met with. In speaking of gold, the author gives Tasmania and Van Dieman's Land as two distinct regions. English and American localities are generally placed as far as possible apart from each other, to prevent the confusion which the similarity of names is apt to occasion. The percentage amount of sulphur in iron pyrites is given as 63,—probably a typographical error.

Principles of Mechanics. By T. M. GOODEVE, M.A., Barrister-at-Law, Lecturer on Applied Mechanics at the Royal School of Mines. London: Longmans, Green, and Co. 1874.

As Prof. Goodeve's "Elements of Mechanism" has so long been established as a standard text-book, we were prepared to receive favourably another work by the same author on a kindred subject. Nor are we disappointed. A glance at the present volume, which forms one of the series of Text-Books now being issued by Messrs. Longmans, is sufficient to show that it will help to sustain the high character which the mechanical and physical part of this series has already acquired.

As Lecturer at the Royal School of Mines, Prof. Goodeve knows well how to treat his subject. The student is made, at the outset, to realise the idea of Force, and to understand the modes by which it may be measured. At an early stage he is brought to grapple with some of the modern views which have been introduced into mechanics, such as the mechanical relations of the theory of heat. The laws of motion and the general principles of dynamical and statical science are clearly enunciated and copiously illustrated. Indeed, the characteristic feature of the present work is the constant reference which is made from the principles of the science to its practice. No sooner has the author laid down a principle than he proceeds to apply it by describing some useful examples, which exhibit its practical application. Hence we find in this work notices of many inventions which we hardly look for in an ordinary treatise on the Principles of Mechanics. We may substantiate our statement by reference to the descriptions of Giffard's Injector, Blake's Stone-Crusher, the Moncrieff Gun-Carriage, Whitworth's Measuring Machine, and many other ingenious mechanical inventions. In fact, the author constantly seeks to interest the student by keeping the practical phase of the subject before him. Prof. Goodeve evidently has no notion of a student learning Mechanics by merely mastering abstract principles and acquiring dexterity in the application of formulæ. If such knowledge is to be any real service to a man, it must be supplemented by practical knowledge, such as may be gained in the workshop. "Mechanics," says Prof. Goodeve (p. 40), "cannot be learnt from books alone. The student must go out into the world, and see how mechanicians and engineers accomplish what they do; he must continually reason upon what he sees, and, retaining a firm hold of mechanical principles, he may thus gradually obtain a knowledge and mastery of his subject."

Elements of Metallurgy. A Practical Treatise on the Art of Extracting Metals from their Ores. By J. ARTHUR PHILLIPS, M. Inst. C.E., F.G.S., F.C.S., &c. Illustrated by numerous Engravings on Wood. London: Charles Griffin and Co. 1874.

RATHER more than twenty years ago Mr. Phillips wrote an excellent "Manual of Metallurgy," which originally formed one of the volumes of the "Encyclopædia Metropolitana." This manual passed through three editions, and a fourth has been long expected. Instead, however, of publishing a new edition of this Manual, the author has followed the far preferable course of presenting us with an entirely independent work. It is proverbially injudicious to put new wine into old bottles, and it has too often been found that to insert a quantity of new matter into an old text-book produces a result which is the very reverse of satisfactory. We are therefore not sorry to see the older work superseded by one which treats the entire subject from a modern point of view, and which may be thoroughly relied upon for giving its information quite up to date. Such a work was much needed, and the need could hardly have been better supplied than it has been by Mr. Phillips.

After a brief sketch of the history of Metallurgy, the author describes the various physical properties of the metals, and then enters into a detailed description of fuels and fire-clays. This is followed by a full notice of each metal, and the methods employed for its extraction, the several metals being treated in the following order:—Iron, cobalt, nickel, aluminium, copper, tin, antimony, arsenic, zinc, mercury, bismuth, silver, gold, and platinum. The details of the metallurgical operations are described with great clearness, and the author's practical experience is sufficient guarantee that space is not wasted by the description of processes which are either obsolete or impracticable. It should be remarked that the illustrations are in most cases drawn to scale, and are consequently of great value to those practically engaged in metallurgy.

In the Preface to this work Mr. Phillips tells us that his object has been to supply such practical information on general principles and typical processes as may not only afford a comprehensive view of the science and art of Metallurgy, but will also enable the reader to study with advantage more elaborate treatises and original memoirs on special departments of the subject. This being the end which Mr. Phillips has set before him, he deserves to be heartily congratulated on its successful realisation.

Le Monde Microscopique des Eaux. Par JULES GIRARD. Paris: J. Rothschild.

A POPULAR treatise on the marvels, animal and vegetal, of pond-life, as revealed by the microscope. The remarks on the construction and use of this instrument err, if at all, on the side of brevity. The work is furnished with a great number of illustrations, some of which, however, are deficient in that precision and accuracy of detail which can alone give any real value, whilst others—*i.e.*, a general view of the vegetation on the margin of a ditch—are rather ornamental than useful.

PROGRESS IN SCIENCE.

MINING.

It might naturally be expected that at the recent meeting of the British Association considerable interest would be created by the iron-mines of Antrim. Within the last few years workings have been opened in all directions throughout the county; and indeed this remarkable development of iron-mining forms an important chapter in the industrial history of the North of Ireland. Not to mention the early notices of these ores, even in the beginning of the seventeenth century, we may remark that the ferruginous bands in the basalt of the Giant's Causeway were observed by the Rev. Dr. Hamilton, and specially mentioned by him, in 1790. No workings, however, were established until 1861, when Dr. Ritchie, of Belfast, opened up the deposit of ore at Ballypalidy, near Templepatrick. Brought into the market under the name of "Belfast aluminous ore," this mineral became largely used for mixing with other ores, especially with the rich hæmatites of Cumberland and Lancashire; the high percentage of free alumina in the Belfast ore acting as a capital flux to the siliceous hæmatite, and contributing to the formation of a good free-flowing slag. The composition of this Irish ore brings it into close relation with the mineral known as Bauxite, which occurs in the South of France and in Carniola, and is employed both as an ore of aluminium and as a fettling material for puddling-furnaces. The remarkable deposits of iron-ore now worked in other parts of Antrim differ for the most part from those of Ballypalidy, and often present a curious pisolitic structure; the spheroids, which are sometimes magnetic, being embedded in a matrix of brown or reddish iron-ochre. This pisolitic ore contains from 30 to 65 per cent of metallic iron, whilst the aluminous ore yields from 20 to 28 per cent of iron: even the ochres and lithomarge, though containing much less iron, have also been used in the blast-furnace. The beds of pisolitic iron-ore appear to lie all on one geological horizon, and thus divide the great series of basaltic rocks into two well-defined groups—one below and the other above the level of the iron-ores. It is probable that the Miocene ores of Antrim may have been formed originally from the products of decomposition of the basalt, and the deposition of these products under lacustrine conditions. The characters and distribution of these ores have been well described by Mr. Ralph Tate and Dr. Sinclair Holden, in the "Journal of the Geological Society;" by Prof. Hull, in "Iron;" and by Mr. R. A. Watson, in the "Dublin University Magazine." A brilliant future for the north-eastern corner of Ireland may be foreseen in the development of these important iron-making resources.

Among the papers read at the recent meeting of the Iron and Steel Institute, at Barrow-in-Furness, we may refer specially to one of local interest, by Mr. P. Würzburger, of Dalton-in-Furness. This memoir, "On the Geology of the West Coast Iron Districts," described the structure of the country which the Institute was then visiting, and dwelt especially on the mode of occurrence of the red hæmatite, which forms the staple of the great iron-manufacture of this district. These ores are found partly as veins in the Lower Silurian rocks, but mainly as deposits in the carboniferous limestone. The ore in the limestone occurs under several conditions: sometimes in flat deposits, following more or less closely the dip of the strata; sometimes in veins, with an inclination greater than that of the enclosing rock; and in other cases in irregular deposits, filling hollows or caverns in the limestone. In the Furness district most of the deposits are covered with superficial drift, but around Whitehaven they are generally enclosed in the solid limestone. In consequence of the great irregularity in the distribution and size of the ore-deposits the mining operations are rather precarious; borings in search of the ore are made at random, and it is impossible to estimate, with anything like an approximation to accuracy, what extent of hæmatite still remains unwrought.

It may be well to remark that a description of the hæmatite deposits of Whitehaven and Furness, by Mr. J. D. Kendall, was laid before the Geological Society of London at its last meeting.

Whilst the occurrence of valuable hæmatite in Furness has led to the establishment of a vast industry in this district, it is to be regretted that coal has not been found in the neighbourhood of the ores. Search has been made from time to time, but hitherto without success. The most promising of these undertakings is that at Rampside, where a boring was commenced about five years ago, and has been energetically prosecuted in the face of great difficulties. Mr. Alexander Brogden, M.P., read a paper on this boring to the Iron and Steel Institute. For some time past the diamond-boring machine has been at work here, and at present the bore-hole has reached a depth of 1450 feet.

Irish coal-mining is carried on to so limited an extent that we may fairly call attention to the Tyrone coal-field, which ought to be developed with great benefit to the North of Ireland. Mr. E. T. Hardman, of the Geological Survey, has recently described this field. It measures only about 2½ miles in length by 1¼ mile in width; but although of so small an area it contains about twenty-four seams, of which at least thirteen are workable. They all consist of highly bituminous coal, of true Carboniferous age; fire-damp is almost unknown in the pits. Mr. Hardman estimates that the field still contains from 30 to 40 millions of tons of coal. Iron-stones are associated with the coal, though not in sufficient quantity to admit of being profitably worked, but the fire-clays in the upper part of the measures are largely used for the manufacture of bricks and tiles.

At a meeting of the Institute of Mining and Mechanical Engineers, recently held at Cardiff, a paper of much interest, "On the Coal-Fields of South Wales," was read by Mr. Forster Brown, the President of the South Wales Institute of Engineers. After tracing the history of iron-smelting in this district, he described in detail the geological structure of this field. Practically the field is divided into two separate parts, by an anticlinal ridge running in a sinuous east and west course. In passing from one part of the field to another, a change may be observed from bituminous coal to anthracite: this change operates both horizontally and vertically; thus, in passing from east to west the coal becomes, as a rule, more anthracite, whilst in other cases the upper seams in a vertical section may be more bituminous than the lower. Mr. Brown contrasted the modes of working coal in South Wales with those followed in the North of England.

To the same meeting a paper "On the Coal-Fields and Mining Industries of Russia was communicated by Mr. J. B. Simpson, who had recently visited some of the Russian collieries. The coal-fields of Russia may be referred to three districts:—the Tula coal-field, at present but little worked; the Donetz field, embracing a great thickness of good coals, partly bituminous and partly anthracitic; and a long narrow coal-field at the base of the Ural Mountains. None of the coal in these districts is of true Carboniferous age, and indeed the only coal-measure fuel belonging to Russia is to be found in a small, but productive, basin in Poland—perhaps an extension of the coal-field of Upper Silesia.

Attention has been directed, by Mr. P. Le Neve Foster, jun., to the coal-fields of Italy. It appears that Italy does not possess any coal of the true Carboniferous period, but many of the tertiary lignites are of very superior quality, and consequently well worth the expense of working. Mr. Foster has examined and described the coal-fields of the Tuscan Maremma, which are split up into a series of small basins by intrusions of eruptive rocks.

Three Reports on the Coal-Fields of Victoria have been drawn up by the Board which was appointed some time ago, by the Minister of Mines, to investigate and report upon the coal resources of the Colony. The fields of

Loutit Bay, Apollo Bay, and the Wannon, have been examined, but the result is by no means encouraging. Mr. Mackenzie, the Government Examiner of Coal-Fields in New South Wales, has reported that—in spite of what may have been said to the contrary—no workable seam of coal has yet been opened up in any part of Victoria. The Board consequently does not feel justified in recommending the expenditure of more money in boring for coal. The most important fuel in the Colony appears to be the lignite of Lal-lal, where a deposit occurs not less than 100 feet in thickness. This lignite has been described before the Geological Society of London, by Mr. R. Etheridge, jun.

But if Victoria is not blessed with coal, it has extraordinary treasures in its gold-fields. A Report on the Mineral Resources of Ballarat, by Mr. R. A. F. Murray, has been issued by the Geological Survey of Victoria. Not only is the geological structure of the country described, but the intricate system of auriferous "leads" has been worked out with much care. The gold-bearing drifts, associated with volcanic rocks which have been erupted at different periods, are classified under four heads. In connection with these drifts it is interesting to study the distribution of the quartz-reefs. It may be regarded as established that the supply of alluvial gold was derived from veins disintegrated during the various drift-periods, and that the gold—except in very fine particles—has not travelled far from the spot where it was separated from its parent-rock. In the district around Ballarat the richest ground is always in the neighbourhood of the quartz-reefs.

METALLURGY.

At the recent meeting of the Royal Cornwall Polytechnic Society a good deal of interest was excited by Mr. King's Patent Magnetic Ore Separator. This apparatus is used for the separation of minerals containing iron, in cases where the iron-ore is associated with other minerals. It is at present employed at the Ballycorkish Mines, in the Isle of Man, where the ore consists of a mixture of galena, blende, and spathose iron-ore. The lead-ore may be readily separated by taking advantage of its high specific gravity; but the blende and spathic ore, being of nearly the same density, could not be properly separated by even the best systems of sizing and mechanical dressing. Hence the introduction of the Magnetic Separator. The stuff, after being crushed, is roasted at a dull red heat in revolving retorts, when the carbonate of iron is decomposed, and a magnetic oxide produced. The roasted ore is then transferred to the hopper of the magnetic apparatus. This consists of a large drum or wheel, about 18 inches in diameter and 10 inches in breadth, furnished within with a system of magnets arranged radially. The mixed ore, in its passage over a series of four of these drums, has its magnetic portion gradually separated by attraction, and the part which escapes the magnetic drum is clean blende. Mr. King proposes to apply his system to the separation of tin-ore from the iron-pyrites, copper-pyrites, and mispickel, with which it is frequently associated in our Cornish mines. When these mixed ores are roasted, the pyrites may be transformed into magnetic pyrites.

"Our Tin Production and Tin Trade" formed the subject of a valuable paper, by Mr. Robert Hunt, F.R.S., recently contributed to the Miners' Association. According to the returns of the Board of Trade, the exports of tin of British production was 114,201 cwts. in 1871, 113,871 cwts. in 1872, and 115,946 cwts. in 1873. Of tin of foreign and colonial production we exported 41,196 cwts. in 1871, 48,634 cwts. in 1872, and 28,869 cwts. in 1873. Cornwall has now to compete not only with the tin production of "the Straits," including the Malacca Peninsula and the islands of Banca and Billiton, but also with the newly-discovered tin districts of Queensland, New South Wales, and Tasmania.

Mr. Crampton's revolving furnace for puddling iron was described by the inventor in a paper recently read before the Iron and Steel Institute. Small coal or slack is utilised as fuel, and is introduced with a current of air in such a way as to ensure perfect combustion, and the consequent absence of smoke. The fuel and air are fed automatically into the revolving furnace in which the puddling is effected, and therefore without the mediation of a separate combustion-chamber. A very intense, though regular, temperature is obtained, and the phosphorus and sulphur are said to be eliminated to a very large extent from the pig-iron. The furnace is recommended not only by its economy of fuel, but also by the rapidity with which the puddling is effected. With a furnace 12 feet in length and 6 feet in diameter, the inventor has puddled pig-iron in an hour and a quarter from the time when it was cold. The revolving portion of the furnace is protected by a water-casing, and the difficulties of contraction and expansion incident to other revolving furnaces are said to be completely conquered.

The other papers bearing on Metallurgy brought before the Institute were for the most part on mechanical subjects. Among these we may mention one by Mr. Holley, of New York, descriptive of the general arrangement and principal details of the plant used in America for rolling steel rails.

MINERALOGY.

In compliment to the late M. Rivot, of the Ecole des Mines, the name of *Rivotite* has been bestowed upon a new mineral, which occurs in irregular masses disseminated through limestone on the west side of the Sierra del Cadi, in Lerida, Spain. It is an amorphous, compact, opaque mineral, varying in colour from yellowish-green to deep greyish-green. From its analysis the following formula may be deduced:—$Sb_2O_5+4(Cu,Ag)O$.

The memory of Dr. Livingstone is to be perpetuated in a new mineral species found at Huitzuco, in the State of Guerrero, Mexico. *Livingstonite* resembles ordinary antimony-glance, or stibnite; but with its lead-grey colour gives a red powder, and is thus distinguished from the antimony-ore. Moreover, M. Mariano Barcena, who has named the species, found it to contain 10 per cent of mercury, and it appears, indeed, that Livingstonite is a double sulphide of mercury and antimony.

Guanovulite is the name under which Dr. F. Wibel proposes to distinguish a pale yellow crystalline body which occurs associated with the eggs sometimes found in deposits of guano. Analysis shows it to be a hydrous sulphate of ammonia and potash, with an acid sulphate of potash.

Prof. Vom Rath, of Bonn, has described a new zeolitic mineral from the granite of the Isle of Elba. *Foresite*, as the new mineral is called, was found near the village of San Piero in Campo, and occurs in association with other zeolites, such as stilbite and heulandite. It is a hydrous silicate of alumina, lime, and soda, crystallising in the orthorhombic system. The name is complimentary to Sig. R. Foresi, of Portoferrajo, who has done much to aid in keeping up an interest in the mineralogy of Elba.

Under the name of *Veszelyite* a new mineral has been described by Prof. Schrauf, of Vienna. It is a hydrated phosphate of copper, presenting a bluish-green colour, and crystallising in the triclinic system. The mineral occurs on garnet, at Morawitza, in the Banat.

In continuation of Mr. E. S. Dana's researches on the crystallography of *Datholite*, he has applied himself to the study of the fine specimens in the K.K. Hof-Mineralien Kabinet at Vienna, and has published his studies in Tschermak's "Mittheilungen."

A valuable series of mineralogical contributions has been submitted to the German Geological Society by Herr Max Bauer, of Berlin, and published in the Society's "Zeitschrift." These studies relate to the rarer forms of garnet, to the physical properties of mica, the optical characters of margarite, the twin-striæ of iron-glance, and some peculiar crystals of smoky quartz from Switzerland.

ENGINEERING—CIVIL AND MECHANICAL.

The past season has, as is usual at that time of the year, been devoted to the meetings of Associations, at which engineering science has been largely represented. Section G of the British Association is devoted to mechanical science: a considerable amount of what is interesting to mechanical engineers finds its place in papers read at the autumnal meeting of the Iron and Steel Institute, as well as at the meetings of the Mechanical Engineers.

British Association.—Professor James Thompson, President of Section G, drew attention in his Address to the important question of railway accidents and the means of their prevention, and he traced the gradual development of progress in mechanical improvements as applied principally to points and signals. As this will form the subject of a separate article in a future number of the "Quarterly Journal of Science," we shall not dwell further upon it on the present occasion. From thence the Professor referred to the subject of steam navigation, the rapid progress of which he attributed mainly to the introduction jointly of the screw propeller, the compound engine, steam jacketting of the cylinders, superheated steam, and the surface condenser. The invention of the screw propeller has enabled steam to be used as an auxiliary motive-power in ocean steamers, for which paddles were inapplicable, whilst the other improvements have tended to an economy in fuel—an essential object in long voyages,—and by the use of which the consumption of coal is often now found to be reduced to about 2 lbs. per indicated horse-power per hour, from having been 4 lbs. or 5 lbs. in good engines in times previous to about twenty years ago. Deep sea sounding was next reviewed, and Sir William Thomson's new machine referred to, with which soundings have been made in the Bay of Biscay, and a specimen of the bottom brought up from a depth of 2700 fathoms, or a little more than three miles. One important feature in this machine consists in a friction brake arrangement, by means of which the arrival of the sinker at the bottom is indicated very exactly on board the ship. The illumination of lighthouses was next considered, the improvements in which were briefly explained. The practicability of employing gas has been one great movement in advance, and Sir William Thomson has recently succeeded in perfecting a self-signalling apparatus, by which different lighthouses may readily be recognised and distinguished from one another, and which is about to be adopted by the Belfast Harbour Commissioners. The Dublin harbour works carried out by Mr. Bindon Stoney, with concrete masonry blocks of 350 tons weight each, to which Mr. Thomson referred, has already been explained in Engineering Chronicles on a former occasion. From these subjects the Professor next branched off into the regions of sanitary engineering, wherein he dwelt principally upon the defective drainage and ventilation of our buildings, and the smoke nuisance of our towns. With reference to the latter subject he pointed out the economy that resulted from the use of well-known mechanical contrivances for the more effectual combustion of coal, and he instanced a case where one method is applied to about thirty ordinary 40 horse-power boilers, in which upwards of 100 tons of coal are burned daily, and from the chimneys of which not more smoke is emitted than from many a kitchen fire. By the adoption of this method the bed of coal, which is gradually supplied in front, is caused to travel along the bars towards the inner end of the furnace, and the combustion proceeds in a very uniform manner in conditions highly favourable to economy of fuel, and without the emission of almost any visible smoke.

We shall notice two papers that were read before this Section, viz., "On the Upper Bann," by John Smyth, and "On the Eclipsing Apparatus at Holywood Lighthouse," by Mr. W. Bottomley.

The former paper gave an account of recent works undertaken for improving the water supply of the river Bann for the mills which are established on its banks; for this purpose, the Bann Reservoir Company was formed in 1835, and by its means the Loughislandreavy reservoir was finished in 1839, and the Corbet reservoir in 1847. Loughislandreavy reservoir is situated in a narrow valley in the flank of the Mourne Mountains, and receives three-fourths of its supply through a conduit or feeder from the Muddock River and one-fourth in the same manner from the Moneyscalp river. The water is impounded to a depth of 35 feet above the old lake which existed there, and can be drawn off to a depth of 38½ feet below top water. The area of the old lake was 93 acres, and that of the present reservoir 250 acres, and the capacity 270 million cubic feet. The embankments are substantially constructed, and protected against the wash of the water by strong stone pitching. An important and most successful speciality in their construction was the use of peat as a supplementary aid to the puddle. The water is discharged through two 18-inch iron pipes, secured in a culvert built in the solid ground beneath the main embankment, and is conducted by an open channel to a point about 1 mile down stream from where the supply is lifted. The water from the reservoir reaches the Bann after flowing down the Muddock for 6 miles. From the mouth of the Muddock to the weir, where the surplus water of the Bann is lifted for the Corbet reservoir, there is about 40 feet of unoccupied, and 7½ feet of occupied, fall on the Bann. At this weir the caretaker daily measures the quantity of water coming down the river, and thereby regulates the amount to be supplemented by the reservoirs. The channel of the feeder from this weir to the Corbet reservoir is about a mile long, and wide enough to take in considerable floods. The area of the reservoir is about 70 acres when the water is at top level, or 11½ feet above discharge outlet. The water is discharged through small iron sluices into a conduit communicating with the river. The cost of these works has been about £30,000.

In the paper upon lighthouse illuminations the author observed that in order, under present arrangements, to make out with certainty what any observed light is, it is necessary that the master of the vessel shall first ascertain the position of his own ship. In many cases this cannot be done even in short voyages, but after a long voyage, and with but few opportunities for making correct observations, errors of many miles may occur in a ship's reckonings. Every year the accounts of shipwrecks show the fatal results arising from the mistake of one light for another light many miles away: the signal which properly interpreted should have preserved the mariner from danger, misinterpreted becomes the false guide which lures him to destruction. Even coloured lights do not afford the required protection, but what is required is that each light should unmistakably declare its own identity; and the plan for effecting this, which was first proposed by Charles Babbage, in 1851, and has recently been perfected by Sir William Thomson, is that each lighthouse shall exhibit from sunset to sunrise a certain definite series of eclipses representing one of the letters of what is known in telegraphy as the Morse alphabet. This method has recently been adopted by the Harbour Commissioners of Belfast for the light on the Holywood Bank, where, by the use of three shutters, or screens, two short eclipses and one long eclipse are given, corresponding to the letter U in the Morse alphabet, and the system will therefore receive a fair trial.

The majority of the papers read before the Iron and Steel Institute at their meeting at Barrow-in-Furness belonged more to geology than engineering, but two of them may be classed under the latter heading—one being a paper by Mr. Robert Luthy, "On Valves Suitable for Working Hydraulic Machinery;" and the other, by Mr. T. Wrightson, of Stockton-on-Tees, "On a New Form of Wagon Drop for Blast-Furnaces."

In the former paper it was explained that under high pressures the old-fashioned taper, or circular, valves are not reliable; brass slide valves of the usual D pattern, similar to steam-engine slide valves, do very well for smaller sizes, and if their working faces are made of hard metal they last a considerable time; but for the larger sizes the friction becomes so great that, in order to work them by hand, a very great leverage has to be employed, and this prevents the valves being opened or closed quickly enough. Mitred plug valves are used in some places, but they are complicated and expensive, and require a great deal of room. The foregoing arrangements of valve have all metal faces, but recently valves have been introduced in which the peculiar properties of the leather collars for making hydraulic joints have been applied to the best advantage. The leathers, it is found, last a very long time, and if a valve has to be examined, or fresh leathers put in, it can be done in a few minutes, whereas the repair of metal-faced valves is both tedious and expensive.

In the ordinary form of wagon drop a frame-work, usually of cast-iron columns, braced well together, supports an entablature, on the top of which is mounted a strong shaft with two large sheaves keyed thereon, to one or both of which is applied a powerful brake, worked by a lever from the upper rail level. The cage moves up and down in guides fixed to the frame-work, and is suspended by chains or wire ropes descending from one side of the sheaves, whilst from the opposite side hang heavy counterweights, which are sufficiently in excess of the weight of the cage to draw it to the top when the wagon is not on. Instead of weights Mr. Wrightson proposes to use water as the controlling agent in the drop. It would occupy too much space to enter into a description of how this is arranged, but it will be readily understood by anyone acquainted with the general principles of hydraulic machinery.

Mr. F. J. Bramwell, as President of the Institution of Mechanical Engineers, recently delivered a very remarkable address on the performances of his profession and the progress made in mechanical science, from which it would seem that the mechanical engineer is necessary to our very existence, and that few modern improvements could have been adopted without his aid, and yet he urged that there was no reason for supposing that the time had arrived when there need be any slackening in the development of engineering science, for there remain vast fields of research in which the ground has as yet scarcely been broken, while there are others practically untouched, and, even in those departments of engineering in which most progress has been made, far more remains to be done than has yet been accomplished. The waste of fuel was dwelt upon, and Mr. Bramwell remarked that for years past there have been constructed steam-engines capable of developing all the power at present obtained by the use of such engines, with the consumption of but little, if any, more than one-third of the fuel actually expended for that purpose, while in our metallurgical and manufacturing processes parallel instances exist. Attention was also directed to the transmission of power over long distances, for which purposes the chief methods adopted are by a fast running rope, by the exhaustion or compression of air, or by the flow of water through pipes under pressure. Amongst other subjects dwelt on were the substitution of mechanical for human labour; the mode of joining materials, and the abvisability of devising some mode less crude than the present practice of employing bolts and rivets; on the utilisation of so-called "waste" products; on the advantages which the present generation of engineers may be expected to derive from the spread of technical education, and on the disadvantages which, on the other hand, are attendant on the modern tendency towards subdivision in engineering manufactures.

The first paper read was one by Mr. John McConnochie, of Cardiff, "On the Bute Docks and the Mechanical Arrangements for Shipping Coal." The dock accommodation at present existing affords a water area of 77 acres, to which will be added a further area of 54 acres when the new Roath Dock is completed. As the tidal water at Cardiff carries a deal of silt in suspension, and is therefore not suited for supplying the docks, they are fed with water drawn from the river Taff about 2 miles above them. In connection with the harbour accommodation there are at present four graving docks. The lock gates, as well as a large swing bridge over a junction lock, and the various hoists, tips, &c., connected with the new basin, are all actuated by hydraulic machinery supplied by Sir W. G. Armstrong and Co. As the preponderance of the trade of the port consists of exports, vessels arrive in ballast, and there are seven steam cranes specially employed in the discharge of this ballast, four of which are capable of discharging 200 tons each per hour. These steam cranes discharge into railway wagons, by which the ballast is conveyed a distance of about 2 miles to spare land, where it is deposited. The mechanical arrangements for shipping coals at the West and East Docks are on the high level, or balanced principle, but at the New Basin the low level system has been adopted into hydraulic tips for elevating and lifting the wagons. The provision for loading coals consists of thirty-one balance tips and twelve hydraulic tips, or forty-three in all; and, as each tip is capable of shipping 560 tons of coal per day of ten hours, the total shipping capacity of the Bute Docks is nearly equal to 8 millions of tons per annum.

We have to record the death, during the past quarter, of more than one eminent member of the Engineering Profession, amongst whom we may note Mr. T. Marr Johnson, Mr. John Grantham, Sir Charles Fox, Sir John Rennie, and Sir William Fairbairn. Sir William Fairbairn was born at Kelso, on the 9th of February, 1789, his parents occupying a comparatively humble position in life. His first occupation was at the age of fourteen, when he obtained employment on the new bridge at Kelso, which was being erected by Mr. Rennie. Afterwards he was employed by his father on the Percy Main Colliery, of which he was manager, and at the age of sixteen he was apprenticed to the Colliery Company, and he commenced a course of self-education. At the age of twenty-one years he found his way to London, where he first obtained work at Grundy's Rope-Factory, at Shadwell, and afterwards was engaged by Mr. Penn, at Greenwich. Afterwards he worked at the Phœnix Foundry, Dublin, and in 1814 he made his way to Manchester, where he settled as a working millwright under Mr. Adam Parkinson. After two years he married, and commenced business on his own account, one of his first efforts at engineering designing being the plans for an iron bridge over the River Irwell, at Blackfriars. Fairbairn gradually rose to a position of eminence, and in 1831 he constructed one of the earliest examples of iron ship-building, the success of which led to the establishment of the well-known works at Millwall. He was engaged with Robert Stephenson in designing and constructing the Britannia Tubular Bridge. In course of time the firm of which Sir William was the leading partner was turned into a Limited Liability Company, and one of the latest works of importance turned out by it was the construction of the iron forts for the defence of Spithead.

GEOLOGY.

Glacial Geology.—The glaciation of the South-West of England has lately attracted some attention. Mr. Croll, speaking of the Great Baltic glacier, 1000 or 2000 feet in thickness, is of opinion that if it be admitted that it passed over Denmark,—and of this there is good geological evidence,—then it is hardly possible to escape the conclusion that a portion of it at least passed across the South of England, entering the Atlantic in the direction of the Bristol Channel. Mr. W. C. Lucy has detected Glacial striæ on a mass of sandstone near Porlock. Mr. H. B. Woodward has recorded the occurrence of Boulder Clay near Yarcombe, on the Black Down hills. Looking to the nature of the deposits found on these hills, he thinks that their formation may be attributed to marine action during the glacial submergence, with the assistance of an occasional iceberg to bring the foreign material (quartz and quartzite) which is mixed with the flint and chert drift.

Mr. T. F. Jamieson has arranged the Glacial phenomena of Scotland under three heads:—(1), the great early glaciation by land-ice; (2), the period of glacial marine beds containing remains of Arctic Mollusca, when most of the country was covered by the sea; (3), the time of the late glaciers, the special subject of the paper. He described this last period as one not of mere local glaciers, but as characterised by a return of a great ice-sheet over nearly the whole of Scotland and Ireland; and he stated that this ice-sheet was probably neither so thick, so extensive, nor so enduring as that of the first period of glaciation, which cleared away everything in the shape of superficial deposits down to the hard rock. He believed, however, that in the last period the mountains of Scotland and Wales, as well as the Pennine range and the rest of the North of England as far as Derby, were covered with thick ice, which in most parts reached down to the sea, and that extensive snow-beds prevailed over the rest of England. In the summer months the melting of these would give rise to streams of muddy water, and produce the superficial deposits of brick-earth, warp, and loess; whilst, when the currents were stronger, perhaps from the thaw being unusually rapid, deposits of gravel would be formed. This second ice-sheet would gradually become less, and break up into valley-glaciers, which in their retreat would leave kaims and eskers at low levels, and moraines in the mountain-glens. During this time no new great submergence of the country took place; and the last great modifications of the surface were subaërial, and not submarine, the work having been done by frost, rain, and glaciers.

Mr. J. G. Goodchild has described the Glacial phenomena of the Eden Valley and the Western part of the Yorkshire Dale district. He considers that they could not possibly have been produced by floating ice, and therefore must have been caused by land-ice. The flow of ice evidently came from the Scottish southern uplands, and it could not well have exceeded 2400 or 2500 feet in thickness.

Stratigraphical Geology.—The exact relations of the Keuper and Bunter divisions of the Trias, in the British Isles, have never been fairly established. Mr. J. Anderson points out that in County Down they appear conformable, whereas near Nottingham the Keuper is said—by the Rev. A. Irving—to rest upon the eroded surface of the Bunter. Mr. Irving has noticed clear signs of continuous deposition of the Permian and Lower Bunter rocks in the Nottingham district; and this fact is important, considering that one series of rocks is classed as Palæozoic, the other as Mesozoic. Mr. H. B. Woodward, referring to the Red Rocks in the South-West of England, remarks upon the continuous series of rocks from the breccias and sandstones of Dawlish and Teignmouth up in to the Rhætic beds of Axmouth. Although the lower portion of these Red beds has been called Bunter, and the upper Keuper, there is no unconformity, and the Muschelkalk may, he thinks, be represented by sediments of a different lithological nature to the continental beds. Nevertheless it is not well to make any subdivisions in this part of England equivalent to those on the Continent, but rather to rest satisfied with terming the whole series the Trias.

Mr. H. G. Seeley having visited the Devonian country in search of the fault which the late Mr. Jukes supposed to traverse it, records his opinion that no such fault exists. He thought that Mr. Etheridge's detailed grouping of the rocks was better suited to the north-west part of the country than West Somerset, and that for that region the divisions of strata used by Mr. Jukes were convenient.

Palæontology.—Mr. E. B. Tawney has described and figured nineteen new species of *Gasteropoda* from the Inferior Oolite of Dundry Hill, near Bristol. He also adds fifteen more species to the list of British fossils: altogether he records sixty-six species in a determinable condition.

Dr. Nicholson has described a new genus of tabulate corals, *Columnopora*. intermediate between *Favosites* and *Columnaria*, of which only a single species has been obtained from the Hudson River Formation of Ontario and Ohio.

The occurrence of Labyrinthodont remains in the Yoredale rocks of Wensleydale has been noted by Mr. L. C. Miall.

Prof. Young and Mr. John Young have described two new forms of Polyzoa from the Carboniferous Limestone shales near Glasgow, under the names of *Actinostoma* and *Glauconome*. Referring also to the genus *Palæocoryne* (described by Dr. Duncan and Mr. H. M. Jenkins), they maintain that the structures are not independent organisms, but mere processes of the Polyzoa on which they occur, the cells at the base being only the cells of the Polyzoa.

Mr. H. G. Seeley has recently described the tibia of a large struthious bird (*Megalornis*) from the London Clay of Eastchurch, in Sheppey. Mr. Seeley has also founded the genus *Ophthalmosaurus* for some reptilian remains from the Oxford Clay.

Sub-Wealden Exploration.—The boring has now reached a depth of upwards of 1000 feet. The beds traversed have been proved to belong to the Lower Wealden, Purbeck and Portland Series, and Kimmeridge Clay. The Oxford Clay has been last detected. The Committee—represented by Prof. Ramsay, Mr. John Evans (President of the Geological Society), and Prof. Prestwich—make an earnest appeal for further assistance to carry on the work.

The Steppes of Siberia.—Mr. Thomas Belt has described some sections examined by him on a portion of the Siberian steppes, which have enabled him to form an opinion as to the origin of the great plain. The best section seen by him was at Pavlodar, which showed 50 feet of sand and silt, with occasional lines of pebbles. South of Pavlodar the surface of the ground was covered with pebbles, which became larger in advancing southward, until the soil was full of large angular quartz boulders. Further south the bed-rock comes to the surface in ridges and low hills, increasing in height until some of them attain 2000 feet. All the rock-surfaces were much shattered, as if by the action of frost, but they showed no signs of glacier-action. The generally accepted marine origin of the great plain was said to be negatived by the absence of sea-shells in its deposits, whilst *Cyrena fluminalis* occurs in them. The author regards them as deposits from a great expanse of fresh water kept back by a barrier of polar ice descending far towards the south. In its greatest extension this ice-barrier would produce the crushing of the bed-rock; and as it retreated, the water coming down from the higher ground in the south would cover a continually increasing surface. The distribution of the boulders on the plain north of the ridges was attributed to floating ice.

Professor of Geology at Oxford.—Mr. Joseph Prestwich, F.R.S., &c., has been appointed to the office of Professor of Geology in the University of Oxford, as successor to the late Prof. Phillips. The Council of the Institution of Civil Engineers have recently awarded to him a Telford Medal and Premium, for his paper "On the Geological Conditions affecting the Construction of a Tunnel between England and France."

Dr. F. Stoliczka.—Palæontological Science has lost one of its ablest students in the death of Dr. Stoliczka, at the early age of 34. He is known chiefly through his connection with the Geological Survey of India, by his descriptions of many of the fossil organic remains collected by the Staff. For several years he was Natural-History Secretary to the Asiatic Society of Bengal, and it is mainly to his exertions that this Society owes the resumption of much of its early vigour.

PHYSICS.

LIGHT.—M. Rayet has published a "Note on the Spectrum of Coggia's Comet." On May 19 the light gave a continuous spectrum from the orange to the blue (spectrum of solid nucleus) traversed by three bright bands (spectrum of gaseous nebulosity); but the continuous spectrum was very narrow compared with the ordinary nuclear spectra; and the luminous bands, instead of being stumped towards the more refrangible side, terminated, towards red and violet, in very distinct straight lines. Father Secchi also writes: On June 18 and 19 the spectrum with the bands of carbon was considerably developed, the green band remaining the more distinct, whilst in the comet of Temple the yellow was brightest. This proves that the gaseous compounds are not the same in all comets. At the beginning of the month there was merely a spectrum of bands, now there is a general connecting line which unites them into one continuous spectrum. The bands of the comet are more diffuse than those of carbonic oxide. They resemble the spectrum obtained by the electric spark in the vapour of benzine.

A new helioscope has been constructed by M. Prazmowski. The idea of employing the polarisation of light in place of coloured glasses to diminish the lustre of the sun is not novel; but the quantity of light reflected, even under the Brewsterian angle for glass of a low index ($n = 1{\cdot}5$) is so considerable that the eye cannot support the light. The author obviates this difficulty by taking a rectangular prism of an index, n, and cementing upon its hypothenuse another similar prism, so as to form a cube. The index of the second prism is n'. A ray of light meets in its course the first hypothenuse of the cemented pair with an incidence of 45°; this is the surface whose index is $\frac{n}{n'}$. As $\frac{n}{n'}$ is very nearly unity, the angle of 45° is very near the Brewsterian incidence. The instrument has been further modified by Janssen.

MICROSCOPY.—Herr Möller has introduced a very ingenious modification of his celebrated *Diatomaceen typenplatte;* the old form had about four hundred species neatly arranged in the space of about a quarter of an inch square, each slide being accompanied by a manuscript catalogue. The new arrangement consists of a photograph about 4 millimetres square, of eighty circles, ten in a longitudinal and eight in a vertical direction; beneath each circle is the name of the object and its author, and in the centre of each of these circles is a diatom, and in many cases two are mounted in order to show front and side views. The whole collection independently of its great value to the student of Diatomaceæ is a marvel of manipulative skill.

Those who have attempted to dry the petals of *Tradescantia Virginica*, have probably noticed that the coloured cell contents break from their envelopes and flow towards the margin of the petals, leaving the greater part in a colourless condition. The petals and their alcoholic tincture giving an interesting absorption spectrum of three bands, it has been a desideratum to preserve the colouring matter in some permanent form for purposes of reference and comparison; the alcoholic tinctures of most flowers rapidly fade, and it is impossible to dry the very fleshy petals of *Tradescantia* in the ordinary way; but the colour has been successfully preserved by mounting in balsam by the "Oil of Cloves" process. The petals separated carefully from the flower so as to avoid breakage or bruises are immersed in ordinary methylated spirit and placed under the receiver of an air pump to ensure rapid displacement of the contained fluid and air; they are next transferred to absolute alcohol, the air pump being also used to save time, then placed in oil of cloves until transparent, and can then be mounted in balsam, taking care that as little heat as possible is employed. Petals of *Cineraria* and *Lobelia speciosa* also make good objects for the micro-spectroscope. The colours are much more brilliant than when dried petals are mounted in balsam, even in those plants which can be successfully dried by the ordinary process.

An exhibition of high scientific value took place at the meeting of the South London Microscopical and Natural History Club. This society has increased so much during the three years of its career that it has been found necessary to seek for a larger meeting-room, in which the first meeting was held on the 21st July. Instead of an indiscriminate collection of objects being exhibited, as at most *conversaziones*, the President, Dr. Braithwaite, F.L.S., determined to limit the exhibition to three subjects, viz., the cuticle of plants and its appendages of hairs, scales, glands, &c.; cyclosis or circulation in plants; and the hard tissues of fruits. A very complete series of preparations was shown, and in addition short lectures on each subject were delivered by the President and two other demonstrators conversant with their respective departments. The educational success of the exhibition was so marked that other meetings illustrating equally important points of minute structure will follow, and probably replace the old fashioned and almost useless *soiree*, which has hitherto been the only means of bringing the microscope and the minute structure revealed by its aid before the general public.

Dr. J. G. Richardson exhibited at the Academy of Natural Sciences of Philadelphia a new "Syphon Slide." It consists of a slip of plate glass 3 inches × 1 inch or other convenient size, in the upper surface of which is ground a shallow groove, elliptical both in its transverse and longitudinal section, and deeper towards one end than the other. This excavation is arranged so as to receive a small living fish, tadpole or other aquatic animal, and retain it under sufficient constraint to prevent any troublesome movements, and also to prevent any injurious pressure. The improvement over other slides of this description consists of imbedding a small metallic tube at each end of the cell, and adapting to each of these two tubes pieces of elastic pipe, one being intended for the entrance and the other for the exit of any fluid which it might be desirable to employ. For the examination of larger animals similar cells of suitable dimensions can be constructed of larger and thicker pieces of glass. With such an apparatus a current of iced water can be passed through the cell, and any injurious effects which might be caused by the use of the electric- or lime-light entirely counteracted. Animals may be kept without injury in these cells for several days so long as a constant current is maintained. This contrivance offers many facilities for prolonged physiological investigations.

TECHNOLOGY.

Composition and analysis of the concentrated milk of the Anglo-Swiss Company of Cham has been determined by M. A. Müntz. The reaction of the milk is feebly alkaline, and its specific gravity is 1·313. Contrary to what has been supposed, a certain quantity of inverted sugar is present. The composition of two samples was—

	No. 1.	No. 2.
Cane-sugar	38·8	29·4
Inverted sugar	1·7	12·4
Milk-sugar	13·3	13·9
Butter	9·5	8·5
Casein, albumen, and salts ..	11·0	12·0
Water	25·7	23·8
	100·0	100·0

The amount of inverted sugar increases with keeping. The milks employed in the manufacture of these two samples must have had the following composition:—

	No. 1.		No. 2.	
Milk-sugar.. ..	5·2	13·2 solids.	5·2	12·8 solids.
Butter..	3·7		3·2	
Casein, &c. ..	4·3		4·4	
Water	86·8		87·2	
	100·0		100·0	

These samples agree closely in their composition with a normal milk, which may be taken as—

Milk-sugar	5·2	13·0
Butter	4·0	
Casein, &c.	3·8	
Water	87·0	
	100·0	

The composition of this milk contrasts very favourably with that generally supplied in Paris, which frequently contains not more than 7 per cent of solids, of which 1·5 to 2 is butter.

According to Ch. Mène, Japan wax has been regularly imported into Europe for some years, and is quoted at from 1¾ to 2 frs. per kilo. It is extensively used for the adulteration of bees'-wax, the value of which ranges from 3¾ to 4 frs. per kilo. This fraud may be detected by the specific gravity of the sample. That of bees'-wax is 0·96931, that of Japan wax 1·00200. Yet all the mixtures containing from 50 to 90 per cent of Japan wax are lighter than bees'-wax.

M. d'Havrincourt collected cockchafers at the price of a franc per 10 litres, and having destroyed them with gas-liquor and sulphuric acid, obtained a good manure. A previous experiment, where the beetles were worked up with lime, gave unsatisfactory results.

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ERRATA.

Page 378, line 1, *read* "motions of the upper currents and of the heavy masses."

Page 383, line 17 from bottom, *read* "It was at this point within 1½ miles of Iowa River, the course of which is here nearly parallel with that of the tornado. The crookedness," &c.

Page 394, line 13, *for* "Smithsonian Institute of Warsaw and Illinois," *read* "Smithsonian Institute, Warsaw, Illinois."